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OTHER DEPARTMENTS

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THEORIES OF DECISION-MAKING IN ECONOMICS AND BEHAVIORAL SCIENCE

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[*Editor's note:* This is the first of eight survey articles on recent developments in economics scheduled for appearance in the *Review* over the next few years. Financial support of the series has been generously provided by the Rockefeller Foundation. The managing editor is particularly grateful for the personal interest which the late Dr. Norman S. Buchanan, Director for the Social Sciences at the Foundation, took in the planning of the project.]

Recent years have seen important new explorations along the boundaries between economics and psychology. For the economist, the immediate question about these developments is whether they include new advances in psychology that can fruitfully be applied to economics. But the psychologist will also raise the converse question—whether there are developments in economic theory and observation that have implications for the central core of psychology. If economics is able to find verifiable and verified generalizations about human economic behavior, then these generalizations must have a place in the more general theories of human behavior to which psychology and sociology aspire. Influence will run both ways.[1]

I. *How Much Psychology Does Economics Need?*

How have psychology and economics gotten along with little relation in the past? The explanation rests on an understanding of the goals toward which economics, viewed as a science and a discipline, has usually aimed.

Broadly speaking, economics can be defined as the science that

* The author is professor of administration at the Carnegie Institute of Technology. This paper draws heavily upon earlier investigations with his colleagues in the Graduate School of Industrial Administration, carried out in library, field, and laboratory, under several grants from the Ford Foundation for research on organizations. He is especially indebted to Julian Feldman, whose wide-ranging exploration of the so-called binary choice experiment [25] has provided an insightful set of examples of alternative approaches to a specific problem of choice.

[1] The influence of economics upon recent work in the psychology of higher mental processes is well illustrated by Bruner, Goodnow and Austin [14, Ch. 3 and 4]. In this work, game theory is used to throw light on the processes of concept formation.

describes and predicts the behavior of several kinds of economic man—notably the consumer and the entrepreneur. While perhaps literally correct, this definition does not reflect the principal focus in the literature of economics. We usually classify work in economics along two dimensions: (a) whether it is concerned with industries and the whole economy (macroeconomics) or with individual economic actors (microeconomics); and (b) whether it strives to describe and explain economic behavior (descriptive economics), or to guide decisions either at the level of public policy (normative macroeconomics) or at the level of the individual consumer or businessman (normative microeconomics).

The profession and literature of economics have been largely preoccupied with normative macroeconomics. Although descriptive macroeconomics provides the scientific base for policy prescription, research emphases have been determined in large part by relevance to policy (e.g., business cycle theory). Normative microeconomics, carried forward under such labels as "management science," "engineering economics," and "operations research," is now a flourishing area of work having an uneasy and ill-defined relation with the profession of economics, traditionally defined. Much of the work is being done by mathematicians, statisticians, engineers, and physical scientists (although many mathematical economists have also been active in it).[2]

This new area, like the old, is normative in orientation. Economists have been relatively uninterested in descriptive microeconomics—understanding the behavior of individual economic agents—except as this is necessary to provide a foundation for macroeconomics. The normative microeconomist "obviously" doesn't need a theory of human behavior: he wants to know how people *ought* to behave, not how they *do* behave. On the other hand, the macroeconomist's lack of concern with individual behavior stems from different considerations. First, he assumes that the economic actor is rational, and hence he makes strong predictions about human behavior without performing the hard work of observing people. Second, he often assumes competition, which carries with it the implication that only the rational survive. Thus, the classical economic theory of markets with perfect competition and rational agents is deductive theory that requires almost no contact with empirical data once its assumptions are accepted.[3]

Undoubtedly there is an area of human behavior that fits these assumptions to a reasonable approximation, where the classical theory

[2] The models of rational decision-making employed in operations research are surveyed in Churchman, Ackoff, and Arnoff [16]; Bowman and Fetter [11]; and Vazsonyi [69].

[3] As an example of what passes for empirical "evidence" in this literature, I cite pp. 22-23 of Friedman's *Essays in Positive Economics* [27], which will amaze anyone brought up in the empirical tradition of psychology and sociology, although it has apparently excited little adverse comment among economists.

with its assumptions of rationality is a powerful and useful tool. Without denying the existence of this area, or its importance, I may observe that it fails to include some of the central problems of conflict and dynamics with which economics has become more and more concerned. A metaphor will help to show the reason for this failure.

Suppose we were pouring some viscous liquid—molasses—into a bowl of very irregular shape. What would we need in order to make a theory of the form the molasses would take in the bowl? How much would we have to know about the properties of molasses to predict its behavior under the circumstances? If the bowl were held motionless, and if we wanted only to predict behavior in equilibrium, we would have to know little, indeed, about molasses. The single essential assumption would be that the molasses, under the force of gravity, would minimize the height of its center of gravity. With this assumption, which would apply as well to any other liquid, and a complete knowledge of the environment—in this case the shape of the bowl—the equilibrium is completely determined. Just so, the equilibrium behavior of a perfectly adapting organism depends only on its goal and its environment; it is otherwise completely independent of the internal properties of the organism.

If the bowl into which we were pouring the molasses were jiggled rapidly, or if we wanted to know about the behavior before equilibrium was reached, prediction would require much more information. It would require, in particular, more information about the properties of molasses: its viscosity, the rapidity with which it "adapted" itself to the containing vessel and moved towards its "goal" of lowering its center of gravity. Likewise, to predict the short-run behavior of an adaptive organism, or its behavior in a complex and rapidly changing environment, it is not enough to know its goals. We must know also a great deal about its internal structure and particularly its mechanisms of adaptation.

If, to carry the metaphor a step farther, new forces, in addition to gravitational force, were brought to bear on the liquid, we would have to know still more about it even to predict behavior in equilibrium. Now its tendency to lower its center of gravity might be countered by a force to minimize an electrical or magnetic potential operating in some lateral direction. We would have to know its relative susceptibility to gravitational and electrical or magnetic force to determine its equilibrium position. Similarly, in an organism having a multiplicity of goals, or afflicted with some kind of internal goal conflict, behavior could be predicted only from information about the relative strengths of the several goals and the ways in which the adaptive processes responded to them.

Economics has been moving steadily into new areas where the power of the classical equilibrium model has never been demonstrated, and

where its adequacy must be considered anew. Labor economics is such an area, oligopoly or imperfect competition theory another, decision-making under uncertainty a third, and the theory of economic development a fourth. In all of these areas the complexity and instability of his environment becomes a central feature of the choices that economic man faces. To explain his behavior in the face of this complexity, the theory must describe him as something more than a featureless, adaptive organism; it must incorporate at least some description of the processes and mechanisms through which the adaptation takes place. Let us list a little more concretely some specific problems of this kind:

(a) The classical theory postulates that the consumer maximizes utility. Recent advances in the theory of rational consumer choice have shown that the existence of a utility function, and its characteristics, if it exists, can be studied empirically.

(b) The growing separation between ownership and management has directed attention to the motivations of managers and the adequacy of the profit-maximization assumption for business firms. So-called human relations research has raised a variety of issues about the motivation of both executives and employees.

(c) When, in extending the classical theory, the assumptions of perfect competition were removed, even the definition of rationality became ambiguous. New definitions had to be constructed, by no means as "obvious" intuitively as simple maximization, to extend the theory of rational behavior to bilateral monopoly and to other bargaining and outguessing situations.

(d) When the assumptions of perfect foresight were removed, to handle uncertainty about the environment, the definition of rationality had to be extended in another direction to take into account prediction and the formation of expectations.

(e) Broadening the definition of rationality to encompass goal conflict and uncertainty made it hard to ignore the distinction between the objective environment in which the economic actor "really" lives and the subjective environment that he perceives and to which he responds. When this distinction is made, we can no longer predict his behavior—even if he behaves rationally—from the characteristics of the objective environment; we also need to know something about his perceptual and cognitive processes.

We shall use these five problem areas as a basis for sorting out some recent explorations in theory, model building, and empirical testing. In Section II, we will examine developments in the theory of utility and consumer choice. In Section III, we will consider somewhat parallel issues relating to the motivation of managers. In Section IV, we will deal with conflict of goals and the phenomena of bargaining. In Section V,

we will survey some of the work that has been done on uncertainty and the formation of expectations. In Section VI, we will explore recent developments in the theory of human problem-solving and other higher mental processes, and see what implications these have for economic decision-making.

II. *The Utility Function*

The story of the re-establishment of cardinal utility, as a consequence of the introduction of uncertainty into the theory of choice, is well known.[4] When Pareto and Slutsky had shown that the theory of consumer demand could be derived from the properties of indifference curves, without postulating a cardinal utility function underlying these curves, it became fashionable to regard utility as an ordinal measure—a ranking of alternatives by preference. Indeed, it could be shown that only ordinal utility had operational status—that the experiments that had been proposed, and even tried in a couple of instances, to measure an individual's utilities by asking him to choose among alternatives could never distinguish between two cardinal utility functions that were ordinally equivalent—that differed only by stretchings and contractions of the unit of measurement.

It was shown by von Neumann and Morgenstern, as a byproduct of their development of the theory of games, that if the choice situation were extended to include choices among uncertain prospects—among lottery tickets, say—cardinal utilities could be assigned to the outcomes in an unequivocal way.[5] Under these conditions, if the subject's behavior was consistent, it was possible to measure cardinally the utilities that different outcomes had for him.

A person who behaved in a manner consistent with the axioms of choice of von Neumann and Morgenstern would act so as to maximize the expected value—the average, weighted by the probabilities of the alternative outcomes of a choice—of his utility. The theory could be tested empirically, however, only on the assumption that the probabilities assigned to the alternatives by the subject were identical with the "objective" probabilities of these events as known to the experimenter. For example, if a subject believed in the gamblers' fallacy, that after a run of heads an unbiased coin would be more likely to fall tails, his choices might appear inconsistent with his utility function, while the real difficulty would lie in his method of assigning probabilities. This

[4] Ward Edwards [23] provides an account of these developments from the psychologist's point of view; Chapter 2 of Luce and Raiffa [43] is an excellent introduction to the "new" utility theory. Arrow [5] contains a nonmathematical survey of this and related topics.

[5] The second edition of von Neumann and Morgenstern [50] contains the first rigorous axiomatic demonstration of this point.

difficulty of "subjective" versus "objective" probability soon came to light when attempts were made to test experimentally whether people behaved in accordance with the predictions of the new utility theory. At the same time, it was discovered that the problem had been raised and solved thirty years earlier by the English philosopher and mathematician Frank Ramsey.[6] Ramsey had shown that, by an appropriate series of experiments, the utilities and subjective probabilities assigned by a subject to a set of uncertain alternatives could be measured simultaneously.

Empirical Studies

The new axiomatic foundations of the theory of utility, which show that it is possible at least in principle to determine empirically whether people "have" utility functions of the appropriate kind, have led to a rash of choice experiments. An experimenter who wants to measure utilities, not merely in principle but in fact, faces innumerable difficulties. Because of these difficulties, most experiments have been limited to confronting the subjects with alternative lottery tickets, at various odds, for small amounts of money. The weight of evidence is that, under these conditions, most persons choose in a way that is reasonably consistent with the axioms of the theory—they behave as though they were maximizing the expected value of utility and as though the utilities of the several alternatives can be measured.[7]

When these experiments are extended to more "realistic" choices—choices that are more obviously relevant to real-life situations—difficulties multiply. In the few extensions that have been made, it is not at all clear that the subjects behave in accordance with the utility axioms. There is some indication that when the situation is very simple and transparent, so that the subject can easily see and remember when he is being consistent, he behaves like a utility maximizer. But as the choices become a little more complicated—choices, for example, among phonograph records instead of sums of money—he becomes much less consistent [21, Ch. 3] [47].[8]

We can interpret these results in either of two ways. We can say that consumers "want" to maximize utility, and that if we present

[6] Ramsey's important essay [57] was sufficiently obscure that it was overlooked until the ideas were rediscovered independently by de Finetti [26]. Valuable notes on the history of the topic together with a thorough formal treatment will be found in the first five chapters of Savage [58].

[7] Some of the empirical evidence is reviewed in [23]. A series of more recent empirical studies is reported in Davidson and Suppes [21].

[8] Some more recent experiments [57a], show a relatively high degree of transitivity. A. G. Papandreou, in a publication I have not yet seen (University of California Publications in Economics) also reports a high degree of transitivity.

them with clear and simple choices that they understand they will do so. Or we can say that the real world is so complicated that the theory of utility maximization has little relevance to real choices. The former interpretation has generally appeared more attractive to economists trained in classical utility theory and to management scientists seeking rules of behavior for normative microeconomics; the latter to behavioral scientists interested in the description of behavior.

Normative Applications

The new utility theory has provided the formal framework for much recent work in mathematical statistics—i.e., statistical decision theory.[9] Similarly (it would be accurate to say "synonymously"), this framework provides the basis for most of the normative models of management science and operations research designed for actual application to the decision-making problems of the firm.[10] Except for some very recent developments, linear programming has been limited to decision-making under certainty, but there have been far-reaching developments of dynamic programming dealing with the maximization of expected values of outcomes (usually monetary outcomes) in situations where future events can be predicted only in terms of probability distributions.[11]

Again, there are at least two distinct interpretations that can be placed on these developments. On the one hand, it can be argued: "Firms would like to maximize profits if they could. They have been limited in doing so by the conceptual and computational difficulties of finding the optimal courses of action. By providing powerful new mathematical tools and computing machines, we now enable them to behave in the manner predicted by Alfred Marshall, even if they haven't been able to in the past." Nature will imitate art and economic man will become as real (and as artificial) as radios and atomic piles.

The alternative interpretation rests on the observation that, even with the powerful new tools and machines, most real-life choices still lie beyond the reach of maximizing techniques—unless the situations are heroically simplified by drastic approximations. If man, according to this interpretation, makes decisions and choices that have some ap-

[9] The systematic development of statistics as decision theory is due largely to A. Wald [70] on the basis of the earlier work of J. Neyman and E. Pearson. Savage [58] carries the development further, erecting the foundations of statistics solidly on utility and probability theory.

[10] This work relates, of course, to profit maximization and cost minimization rather than utility maximization, but it is convenient to mention it at this point. See [11] [16] [69].

[11] Arrow, Harris and Marschak [3] were among the first to treat inventory decisions dynamically. A general treatment of the theory of dynamic programming will be found in Bellman [9].

pearance of rationality, rationality in real life must involve something simpler than maximization of utility or profit. In Section VI, we will see where this alternative interpretation leads.

The Binary Choice Experiment

Much recent discussion about utility has centered around a particularly simple choice experiment. This experiment, in numerous variants, has been used by both economists and psychologists to test the most diverse kinds of hypotheses. We will describe it so that we can use it as a common standard of comparison for a whole range of theories and empirical studies.[12]

We will call the situation we are about to describe the *binary choice* experiment. It is better known to most game theorists—particularly those located not far from Nevada—as a two-armed bandit; and to most psychologists as a partial reinforcement experiment. The subject is required, in each of a series of trials, to choose one or the other of two symbols—say, plus or minus. When he has chosen, he is told whether his choice was "right" or "wrong," and he may also receive a reward (in psychologist's language, a reinforcement) for "right" choices. The experimenter can arrange the schedule of correct responses in a variety of ways. There may be a definite pattern, or they may be randomized. It is not essential that one and only one response be correct on a given trial: the experimenter may determine that both or neither will be correct. In the latter case the subject may or may not be informed whether the response he did not choose would have been correct.

How would a utility-maximizing subject behave in the binary choice experiment? Suppose that the experimenter rewarded "plus" on one-third of the trials, determined at random, and "minus" on the remaining two-thirds. Then a subject, provided that he believed the sequence was random and observed that minus was rewarded twice as often as plus, should always, rationally, choose minus. He would find the correct answer two-thirds of the time, and more often than with any other strategy.

Unfortunately for the classical theory of utility in its simplest form, few subjects behave in this way. The most commonly observed behavior is what is called *event matching*.[13] The subject chooses the two alternatives (not necessarily at random) with relative frequencies roughly proportional to the relative frequencies with which they are rewarded.

[12] My understanding of the implications of the binary choice experiment owes much to conversations with Julian Feldman, and to his unpublished work on the experiment. See also, Bush and Mosteller [15] particularly Chapter 13.

[13] An example of data consistent with event-matching behavior is given on page 283 of [15].

Thus, in the example given, two-thirds of the time he would choose minus, and as a result would make a correct response, on the average, in 5 trials out of 9 (on two-thirds of the trials in which he chooses minus, and one-third of those in which he chooses plus).[14]

All sorts of explanations have been offered for the event-matching behavior. The simplest is that the subject just doesn't understand what strategy would maximize his expected utility; but with adult subjects in a situation as transparent as this one, this explanation seems far-fetched. The alternative explanations imply either that the subject regards himself as being engaged in a competitive game with the experimenter (or with "nature" if he accepts the experimenter's explanation that the stimulus is random), or that his responses are the outcome of certain kinds of learning processes. We will examine these two types of explanation further in Sections IV and V respectively. The important conclusion at this point is that even in an extremely simple situation, subjects do not behave in the way predicted by a straightforward application of utility theory.

Probabilistic Preferences

Before we leave the subject of utility, we should mention one recent important development. In the formalizations mentioned up to this point, probabilities enter only into the estimation of the consequences that will follow one alternative or another. Given any two alternatives, the first is definitely preferable to the second (in terms of expected utility), or the second to the first, or they are strictly indifferent. If the same pair of alternatives is presented to the subject more than once, he should always prefer the same member of the pair.

One might think this requirement too strict—that, particularly if the utility attached to one alternative were only slightly greater or less than that attached to the other, the subject might vacillate in his choice. An empirical precedent for such vacillation comes not only from casual observation of indecision but from analogous phenomena in the psychophysical laboratory. When subjects are asked to decide which of two weights is heavier, the objectively heavier one is chosen more often than the lighter one, but the relative frequency of choosing the heavier approaches one-half as the two weights approach equality. The probability that a subject will choose the objectively heavier weight depends, in general, on the ratio of the two weights.

Following several earlier attempts, a rigorous and complete axiom system for a utility theory incorporating probabilistic preferences has been constructed recently by Duncan Luce [cf. 43, App. 1]. Although

[14] Subjects tend to choose the more highly rewarded alternative slightly more frequently than is called for by event matching. Hence, the actual behavior tends to be some kind of average between event matching and the optimal behavior. See [15, Ch. 13].

the theory weakens the requirements of consistency in preference, it is empirically testable, at least in principle. Conceptually, it provides a more plausible interpretation of the notion of "indifference" than does the classical theory.

III. *The Goals of Firms*

Just as the central assumption in the theory of consumption is that the consumer strives to maximize his utility, so the crucial assumption in the theory of the firm is that the entrepreneur strives to maximize his residual share—his profit. Attacks on this hypothesis have been frequent.[15] We may classify the most important of these as follows:

(a) The theory leaves ambiguous whether it is short-run or long-run profit that is to be maximized.

(b) The entrepreneur may obtain all kinds of "psychic income" from the firm, quite apart from monetary rewards. If he is to maximize his utility, then he will sometimes balance a loss of profits against an increase in psychic income. But if we allow "psychic income," the criterion of profit maximization loses all of its definiteness.

(c) The entrepreneur may not care to maximize, but may simply want to earn a return that he regards as satisfactory. By sophistry and and adept use of the concept of psychic income, the notion of seeking a satisfactory return can be translated into utility maximizing but not in any operational way. We shall see in a moment that "satisfactory profits" is a concept more meaningfully related to the psychological notion of aspiration levels than to maximization.

(d) It is often observed that under modern conditions the equity owners and the active managers of an enterprise are separate and distinct groups of people, so that the latter may not be motivated to maximize profits.

(e) Where there is imperfect competition among firms, maximizing is an ambiguous goal, for what action is optimal for one firm depends on the actions of the other firms.

In the present section we shall deal only with the third of these five issues. The fifth will be treated in the following section; the first, second, and fourth are purely empirical questions that have been discussed at length in the literature; they will be considered here only for their bearing on the question of satisfactory profits.

Satisficing versus Maximizing

The notion of satiation plays no role in classical economic theory, while it enters rather prominently into the treatment of motivation in psychology. In most psychological theories the motive to act stems from

[15] For a survey of recent discussions see Papandreou [55].

drives; and action terminates when the drive is satisfied. Moreover, the conditions for satisfying a drive are not necessarily fixed, but may be specified by an aspiration level that itself adjusts upward or downward on the basis of experience.

If we seek to explain business behavior in the terms of this theory, we must expect the firm's goals to be not maximizing profit, but attaining a certain level or rate of profit, holding a certain share of the market or a certain level of sales. Firms would try to "satisfice" rather than to maximize.[16]

It has sometimes been argued that the distinction between satisficing and maximizing is not important to economic theory. For in the first place, the psychological evidence on individual behavior shows that aspirations tend to adjust to the attainable. Hence in the long run, the argument runs, the level of aspiration and the attainable maximum will be very close together. Second, even if some firms satisficed, they would gradually lose out to the maximizing firms, which would make larger profits and grow more rapidly than the others.

These are, of course, precisely the arguments of our molasses metaphor, and we may answer them in the same way that we answered them earlier. The economic environment of the firm is complex, and it changes rapidly; there is no a priori reason to assume the attainment of long-run equilibrium. Indeed, the empirical evidence on the distribution of firms by size suggests that the observed regularities in size distribution stem from the statistical equilibrium of a population of adaptive systems rather than the static equilibrium of a population of maximizers.[17]

Models of satisficing behavior are richer than models of maximizing behavior, because they treat not only of equilibrium but of the method of reaching it as well. Psychological studies of the formation and change of aspiration levels support propositions of the following kinds.[18] (a) When performance falls short of the level of aspiration, search behavior (particularly search for new alternatives of action) is induced. (b) At the same time, the level of aspiration begins to adjust itself downward until goals reach levels that are practically attainable. (c) If the two mechanisms just listed operate too slowly to adapt aspirations to performance, emotional behavior—apathy or aggression, for example—will replace rational adaptive behavior.

[16] A comparison of satisficing with maximizing models of decision-making can be found in [64, Ch. 14]. Katona [40] has independently made similar comparisons of economic and psychological theories of decision.

[17] Simon and Bonini [66] have constructed a stochastic model that explains the observed data on the size distributions of business firms.

[18] A standard psychological reference on aspiration levels is [42]. For applications to economics, see [61] and [45] (in the latter, consult the index under "aspiration levels").

The aspiration level defines a natural zero point in the scale of utility—whereas in most classical theories the zero point is arbitrary. When the firm has alternatives open to it that are at or above its aspiration level, the theory predicts that it will choose the best of those known to be available. When none of the available alternatives satisfies current aspirations, the theory predicts qualitatively different behavior: in the short run, search behavior and the revision of targets; in the longer run, what we have called above emotional behavior, and what the psychologist would be inclined to call neurosis.[19]

Studies of Business Behavior

There is some empirical evidence that business goals are, in fact, stated in satisficing terms.[20] First, there is the series of studies stemming from the pioneering work of Hall and Hitch that indicates that businessmen often set prices by applying a standard markup to costs. Some economists have sought to refute this fact, others to reconcile it—if it is a fact—with marginalist principles. The study of Earley [22a, pp. 44-70] belongs to the former category, but its evidence is suspect because the questions asked of businessmen are leading ones—no one likes to admit that he would accept less profit if he could have more. Earley did not ask his respondents how they determined marginal cost and marginal revenue, how, for example, they estimated demand elasticities.

Another series of studies derived from the debate over the Keynesian doctrine that the amount of investment was insensitive to changes in the rate of interest. The general finding in these studies has been that the rate of interest is not an important factor in investment decisions [24] [39, Ch. 11] [71]. not so

More recently, my colleagues Cyert and March, have attempted to test the satisficing model in a more direct way [19]. They found in one industry some evidence that firms with a declining share of market strove more vigorously to increase their sales than firms whose shares of the market were steady or increasing.

Aspirations in the Binary Choice Experiment

Although to my knowledge this has not been done, it would be easy to look for aspiration-level phenomena in the binary choice experiment.

[19] Lest this last term appear fanciful I should like to call attention to the phenomena of panic and broken morale, which are well known to observers of the stock market and of organizations but which have no reasonable interpretation in classical utility theory. I may also mention that psychologists use the theory described here in a straightforward way to produce experimental neurosis in animal and human subjects.

[20] A comprehensive bibliography of empirical work prior to 1950 will be found in [37]. Some of the more recent work is [19] [24] [39, Ch. 11].

By changing the probabilities of reward in different ways for different groups of subjects, we could measure the effects of these changes on search behavior—where amount of search would be measured by changes in the pattern of responses.

Economic Implications

It has sometimes been argued that, however realistic the classical theory of the firm as a profit maximizer, it is an adequate theory for purposes of normative macroeconomics. Mason, for example, in commenting on Papandreou's essay on "Problems in the Theory of the Firm" [55, pp. 183-222] says, "The writer of this critique must confess a lack of confidence in the marked superiority, *for purposes of economic analysis*, of this newer concept of the firm over the older conception of the entrepreneur." The italics are Mason's.

The theory of the firm is important for welfare economics—e.g., for determining under what circumstances the behavior of the firm will lead to efficient allocation of resources. The satisficing model vitiates all the conclusions about resource allocation that are derivable from the maximizing model when perfect competition is assumed. Similarly, a dynamic theory of firm sizes, like that mentioned above, has quite different implications for public policies dealing with concentration than a theory that assumes firms to be in static equilibrium. Hence, welfare economists are justified in adhering to the classical theory only if: (a) the theory is empirically correct as a description of the decision-making process; or (b) it is safe to assume that the system operates in the neighborhood of the static equilibrium. What evidence we have mostly contradicts both assumptions.

IV. *Conflict of Interest*

Leaving aside the problem of the motivations of hired managers, conflict of interest among economic actors creates no difficulty for classical economic theory—indeed, it lies at the very core of the theory—so long as each actor treats the other actors as parts of his "given" environment, and doesn't try to predict their behavior and anticipate it. But when this restriction is removed, when it is assumed that a seller takes into account the reactions of buyers to his actions, or that each manufacturer predicts the behaviors of his competitors—all the familiar difficulties of imperfect competition and oligopoly arise.[21]

The very assumptions of omniscient rationality that provide the basis for deductive prediction in economics when competition is present lead

[21] There is by now a voluminous literature on the problem. The difficulties in defining rationality in competitive situations are well stated in the first chapter of von Neumann and Morgenstern [50].

to ambiguity when they are applied to competition among the few. The central difficulty is that rationality requires one to outguess one's opponents, but not to be outguessed by them, and this is clearly not a consistent requirement if applied to all the actors.

Game Theory

Modern game theory is a vigorous and extensive exploration of ways of extending the concept of rational behavior to situations involving struggle, outguessing, and bargaining. Since Luce and Raiffa [43] have recently provided us with an excellent survey and evaluation of game theory, I shall not cover the same ground here.[22] I concur in their general evaluation that, while game theory has greatly clarified the issues involved, it has not provided satisfactory solutions. Not only does it leave the definition of rational conduct ambiguous in all cases save the zero-sum two-person game, but it requires of economic man even more fantastic reasoning powers than does classical economic theory.[23]

Power and Bargaining

A number of exploratory proposals have been put forth as alternatives to game theory—among them Galbraith's notion of countervailing power [30] and Schelling's bargaining theory [59] [60]. These analyses draw at least as heavily upon theories of power and bargaining developed initially to explain political phenomena as upon economic theory. They do not lead to any more specific predictions of behavior than do game-theoretic approaches, but place a greater emphasis upon description and actual observation, and are modest in their attempt to derive predictions by deductive reasoning from a few "plausible" premises about human behavior.

At least four important areas of social science and social policy, two of them in economics and two more closely related to political science, have as their central concern the phenomena of power and the processes of bargaining: the theory of political parties, labor-management relations, international politics, and oligopoly theory. Any progress in the basic theory applicable to one of these is certain to be of almost equal importance to the others. A growing recognition of their common concern is evidenced by the initiation of a new cross-disciplinary journal, *Journal of Conflict Resolution*.

[22] Chapters 5 and 6 of [43] provide an excellent survey of the attempts that have been made to extend the theory of games to the kinds of situations most relevant to economics.

[23] In a forthcoming volume on *Strategy and Market Structure*, Martin Shubik approaches the topics of imperfect competition and oligopoly from the standpoint of the theory of games.

Games against Nature

While the binary choice experiment is basically a one-person game, it is possible to interpret it as a "game against nature," and hence to try to explain it in game-theoretic terms. According to game theory, the subject, if he believes in a malevolent nature that manipulates the dice against him, should minimax his expected utility instead of maximizing it. That is, he should adopt the course of action that will maximize his expected utility under the assumption that nature will do her worst to him.

Minimaxing expected utility would lead the subject to call plus or minus at random and with equal probability, regardless of what the history of rewards has been. This is something that subjects demonstrably do not do.

However, it has been suggested by Savage [58] and others that people are not as interested in maximizing utility as they are in minimizing regret. "Regret" means the difference between the reward actually obtained and the reward that could have been obtained with perfect foresight (actually, with perfect hindsight!). It turns out that minimaxing regret in the binary choice experiment leads to event-matching behavior [64, Ch. 16]. Hence, the empirical evidence is at least crudely consistent with the hypothesis that people play against nature by minimaxing regret. We shall see, however, that event-matching is also consistent with a number of other rules of behavior that seem more plausible on their face; hence we need not take the present explanation too seriously—at least I am not inclined to do so.

V. *The Formation of Expectations*

While the future cannot enter into the determination of the present, expectations about the future can and do. In trying to gain an understanding of the saving, spending, and investment behavior of both consumers and firms, and to make short-term predictions of this behavior for purposes of policy-making, economists have done substantial empirical work as well as theorizing on the formation of expectations.

Empirical Studies

A considerable body of data has been accumulated on consumers' plans and expectations from the Survey of Consumer Finances, conducted for the Board of Governors of the Federal Reserve System by the Survey Research Center of the University of Michigan [39, Ch. 5]. These data, and similar data obtained by others, begin to give us some information on the expectations of consumers about their own incomes, and the predictive value of their expenditure plans for their actual sub-

sequent behavior. Some large-scale attempts have been made, notably by Modigliani and Brumberg [48, pp. 388-436] and, a little later, by Friedman [28] to relate these empirical findings to classical utility theory. The current empirical research on businessmen's expectations is of two main kinds:

1. Surveys of businessmen's own forecasts of business and business conditions in the economy and in their own industries [24, pp. 165-88] [29, pp. 189-98]. These are obtained by straightforward questionnaire methods that assume, implicitly, that businessmen can and do make such forecasts. In some uses to which the data are put, it is also assumed that the forecasts are used as one basis for businessmen's actions.

2. Studies of business decisions and the role of expectations in these decisions—particularly investment and pricing decisions. We have already referred to studies of business decisions in our discussion of the goals of the firm.[24]

Expectations and Probability

The classical way to incorporate expectations into economic theory is to assume that the decision-maker estimates the joint probability distribution of future events.[25] He can then act so as to maximize the expected value of utility or profit, as the case may be. However satisfying this approach may be conceptually, it poses awkward problems when we ask how the decision-maker actually estimates the parameters of the joint probability distribution. Common sense tells us that people don't make such estimates, nor can we find evidence that they do by examining actual business forecasting methods. The surveys of businessmen's expectations have never attempted to secure such estimates, but have contented themselves with asking for point predictions—which, at best, might be interpreted as predictions of the means of the distributions.

It has been shown that under certain special circumstances the mean of the probability distribution is the only parameter that is relevant for decision—that even if the variance and higher moments were known to the rational decision-maker, he would have no use for them.[26] In these cases, the arithmetic mean is actually a certainty equivalent, the optimal decision turns out to be the same as if the future were known with certainty. But the situations where the mean is a certainty equival-

[24] See the references cited [12, p. 160].

[25] A general survey of approaches to decision-making under uncertainty will be found in [2] and in [43, Ch. 13].

[26] The special case in which mean expectations constitute a certainty equivalent is treated in [62]. An alternative derivation, and fuller discussion is given by Theil [67, Ch. 8, sect. 6].

ent are, as we have said, very special ones, and there is no indication that businessmen ever ask whether the necessary conditions for this equivalence are actually met in practice. They somehow make forecasts in the form of point predictions and act upon them in one way or another.

The "somehow" poses questions that are important for business cycle theory, and perhaps for other problems in economics. The way in which expectations are formed may affect the dynamic stability of the economy, and the extent to which cycles will be amplified or damped. Some light, both empirical and theoretical, has recently been cast on these questions. On the empirical side, attempts have been made: (a) to compare businessmen's forecasts with various "naïve" models that assume the future will be some simple function of the recent past, and (b) to use such naïve models themselves as forecasting devices.

The simplest naïve model is one that assumes the next period will be exactly like the present. Another assumes that the change from present to next period will equal the change from last period to present; a third, somewhat more general, assumes that the next period will be a weighted average of recent past periods. The term "naïve model" has been applied loosely to various forecasting formulae of these general kinds. There is some affirmative evidence that business forecasts fit such models. There is also evidence that elaboration of the models beyond the first few steps of refinement does not much improve prediction; see, for example, [20]. Arrow and his colleagues [4] have explored some of the conditions under which forecasting formulae will, and will not, introduce dynamic instability into an economic system that is otherwise stable. They have shown, for example, that if a system of multiple markets is stable under static expectations, it is stable when expectations are based on a moving average of past values.

The work on the formation of expectations represents a significant extension of classical theory. For, instead of taking the environment as a "given," known to the economic decision-maker, it incorporates in the theory the processes of acquiring knowledge about that environment. In doing so, it forces us to include in our model of economic man some of his properties as a learning, estimating, searching, information-processing organism [65].

The Cost of Information

There is one way in which the formation of expectations might be reincorporated in the body of economic theory: by treating information-gathering as one of the processes of production, so to speak, and applying to it the usual rules of marginal analysis. Information, says price theory, should be gathered up to the point where the incremental

cost of additional information is equal to the incremental profit that can be earned by having it. Such an approach can lead to propositions about optimal amounts of information-gathering activity and about the relative merits of alternative information-gathering and estimating schemes.[27]

This line of investigation has, in fact, been followed in statistical decision theory. In sampling theory we are concerned with the optimal size of sample (and in the special and ingenious case of sequential sampling theory, with knowing when to stop sampling), and we wish to evaluate the efficiencies of alternative sampling procedures. The latter problem is the simpler, since it is possible to compare the relative costs of alternative schemes that have the same sampling error, and hence to avoid estimating the value of the information.[28] However, some progress has been made also toward estimating the value of improved forecast accuracy in situations where the forecasts are to be used in applying formal decision rules to choice situations.[29]

The theory of teams developed by Marschak and Radner is concerned with the same problem (see, e.g., [46]) It considers situations involving decentralized and interdependent decision-making by two or more persons who share a common goal and who, at a cost, can transmit information to each other about their own actions or about the parts of the environment with which they are in contact. The problem then is to discover the optimal communication strategy under specified assumptions about communication costs and payoffs.

The cost of communication in the theory of teams, like the cost of observations in sampling theory, is a parameter that characterizes the economic actor, or the relation of the actor to his environment. Hence, while these theories retain, in one sense, a classical picture of economic man as a maximizer, they clearly require considerable information about the characteristics of the actor, and not merely about his environment. They take a long stride toward bridging the gap between the traditional concerns of economics and the concerns of psychology.

Expections in the Binary Choice Experiment

I should like to return again to the binary choice experiment, to see what light it casts on the formation of expectations. If the subject is told by the experimenter that the rewards are assigned at random, if he

[27] Fundamental and applied research are examples of economically significant information-gathering activities. Griliches [34] has recently made an attempt to estimate the economic return from research on hybrid corn.

[28] Modern treatments of sampling theory, like Cochran [17] are based on the idea of minimizing the cost of obtaining a fixed amount of information.

[29] For the theory and an application to macroeconomics, see Theil [67, Ch. 8, sects. 5 and 6].

is told what the odds are for each alternative, *and if he believes the experimenter*, the situation poses no forecasting problem. We have seen, however, that the behavior of most subjects is not consistent with these assumptions.

How would sequential sampling theory handle the problem? Each choice the subject makes now has two consequences: the immediate reward he obtains from it, and the increment of information it provides for predicting the future rewards. If he thinks only of the latter consequences, he is faced with the classical problem of induction: to estimate the probability that an event will occur in the future on the basis of its frequency of occurrence in the past. Almost any rule of induction would require a rational (maximizing) subject to behave in the following general manner: to sample the two alternatives in some proportion to estimate the probability of reward associated with each; after the error of estimate had been reduced below some bound, always to choose the alternative with the higher probability of reward. Unfortunately, this does not appear to be what most subjects do.

If we give up the idea of maximization, we can make the weaker assumption that the subject is adaptive—or learns—but not necessarily in any optimal fashion. What do we mean by adaptation or learning? We mean, gradually and on the basis of experience responding more frequently with the choice that, in the past, has been most frequently rewarded. There is a whole host of rules of behavior possessing this characteristic. Postulate, for example, that at each trial the subject has a certain probability of responding "plus," and the complementary probability of responding "minus." Postulate further that when he makes a particular response the probability of making the same response on the next trial is increased if the response is rewarded and decreased if the response is not rewarded. The amount of increment in the response probability is a parameter characterizing the learning rate of the particular subject. Almost all schemes of this kind produce asymptotic behaviors, as the number of trials increases, that are approximately event-matching in character.

Stochastic learning models, as the processes just described are usually called, were introduced into psychology in the early 1950's by W. K. Estes and Bush and Mosteller [15] and have been investigated extensively since that time. The models fit some of the gross features of the observed behaviors—most strikingly the asymptotic probabilities—but do not explain very satisfactorily the fine structure of the observations.

Observation of subjects in the binary choice experiment reveals that usually they not only refuse to believe that (or even to act as if) the reward series were random, but in fact persist over many trials in

searching for systematic patterns in the series. To account for such behavior, we might again postulate a learning model, but in this case a model in which the subject does not react probabilistically to his environment, but forms and tests definite hypotheses about systematic patterns in it. Man, in this view, is not only a learning animal; he is a pattern-finding and concept-forming animal. Julian Feldman [25] has constructed theories of this kind to explain the behavior of subjects in the binary choice experiment, and while the tests of the theories are not yet completed, his findings look exceedingly promising.

As we move from maximizing theories, through simple stochastic learning theories, to theories involving pattern recognition our model of the expectation-forming processes and the organism that performs it increases in complexity. If we follow this route, we reach a point where a theory of behavior requires a rather elaborate and detailed picture of the rational actor's cognitive processes.

VI. *Human Cognition and Economics*

All the developments we have examined in the preceding four sections have a common theme: they all involve important modifications in the concept of economic man and, for the reasons we have stated, modifications in the direction of providing a fuller description of his characteristics. The classical theory is a theory of a man choosing among fixed and known alternatives, to each of which is attached known consequences. But when perception and cognition intervene between the decision-maker and his objective environment, this model no longer proves adequate. We need a description of the choice process that recognizes that alternatives are not given but must be sought; and a description that takes into account the arduous task of determining what consequences will follow on each alternative [63, Ch. 5] [64, Part 4] [14].

The decision-maker's information about his environment is much less than an approximation to the real environment. The term "approximation" implies that the subjective world of the decision-maker resembles the external environment closely, but lacks, perhaps, some fineness of detail. In actual fact the perceived world is fantastically different from the "real" world. The differences involve both omissions and distortions, and arise in both perception and inference. The sins of omission in perception are more important than the sins of commission. The decision-maker's model of the world encompasses only a minute fraction of all the relevant characteristics of the real environment, and his inferences extract only a minute fraction of all the information that is present even in his model.

Perception is sometimes referred to as a "filter." This term is as

misleading as "approximation," and for the same reason: it implies that what comes through into the central nervous system is really quite a bit like what is "out there." In fact, the filtering is not merely a passive selection of some part of a presented whole, but an active process involving attention to a very small part of the whole and exclusion, from the outset, of almost all that is not within the scope of attention.

Every human organism lives in an environment that generates millions of bits of new information each second, but the bottleneck of the perceptual apparatus certainly does not admit more than 1,000 bits per second, and probably much less. Equally significant omissions occur in the processing that takes place when information reaches the brain. As every mathematician knows, it is one thing to have a set of differential equations, and another thing to have their solutions. Yet the solutions are logically implied by the equations—they are "all there," if we only knew how to get to them! By the same token, there are hosts of inferences that *might* be drawn from the information stored in the brain that are not in fact drawn. The consequences implied by information in the memory become known only through active information-processing, and hence through active selection of particular problem-solving paths from the myriad that might have been followed.

In this section we shall examine some theories of decision-making that take the limitations of the decision-maker and the complexity of the environment as central concerns. These theories incorporate some mechanisms we have already discussed—for example, aspiration levels and forecasting processes—but go beyond them in providing a detailed picture of the choice process.

A real-life decision involves some goals or values, some facts about the environment, and some inferences drawn from the values and facts. The goals and values may be simple or complex, consistent or contradictory; the facts may be real or supposed, based on observation or the reports of others; the inferences may be valid or spurious. The whole process may be viewed, metaphorically, as a process of "reasoning," where the values and facts serve as premises, and the decision that is finally reached is inferred from these premises [63]. The resemblance of decision-making to logical reasoning is only metaphorical, because there are quite different rules in the two cases to determine what constitute "valid" premises and admissible modes of inference. The metaphor is useful because it leads us to take the individual *decision premise* as the unit of description, hence to deal with the whole interwoven fabric of influences that bear on a single decision—but without being bound by the assumptions of rationality that limit the classical theory of choice.

Rational Behavior and Role Theory

We can find common ground to relate the economist's theory of decision-making with that of the social psychologist. The latter is particularly interested, of course, in social influences on choice, which determine the *role* of the actor. In our present terms, a role is a social prescription of some, but not all, of the premises that enter into an individual's choices of behavior. Any particular concrete behavior is the resultant of a large number of premises, only some of which are prescribed by the role. In addition to role premises there will be premises about the state of the environment based directly on perception, premises representing beliefs and knowledge, and idiosyncratic premises that characterize the personality. Within this framework we can accommodate both the rational elements in choice, so much emphasized by economics, and the nonrational elements to which psychologists and sociologists often prefer to call attention.

Decision Premises and Computer Programs

The analysis of choice in terms of decision premises gives us a conceptual framework for describing and explaining the process of deciding. But so complex is the process that our explanations of it would have remained schematic and hypothetical for a long time to come had not the modern digital computer appeared on the scene. The notion of decision premise can be translated into computer terminology, and when this translation has been accomplished, the digital computer provides us with an instrument for simulating human decision processes—even very complex ones—and hence for testing empirically our explanations of those processes [53].

A fanciful (but only slightly fanciful) example will illustrate how this might be done. Some actual examples will be cited presently. Suppose we were to construct a robot incorporating a modern digital computer, and to program (i.e., to instruct) the robot to take the role of a business executive in a specified company. What would the program look like? Since no one has yet done this, we cannot say with certainty, but several points are fairly clear. The program would not consist of a list of prescribed and proscribed behaviors, since what an executive does is highly contingent on information about a wide variety of circumstances. Instead, the program would consist of a large number of *criteria* to be applied to possible and proposed courses of action, of routines for *generating* possible courses of action, of computational procedures for *assessing* the state of the environment and its implications for action, and the like. Hence, the program—in fact, a role prescription—would interact with information to produce concrete behavior adapted to the situation. The elements of such a program take

the form of what we have called decision premises, and what the computer specialists would call instructions.

The promise of constructing actual detailed descriptions of concrete roles and decision processes is no longer, with the computer, a mere prospectus to be realized at some undefined future date. We can already provide actual examples, some of them in the area of economics.

1. *Management Science*. In the paragraphs on normative applications in Section II, we have already referred to the use of such mathematical techniques as linear programming and dynamic programming to construct formal decision processes for actual situations. The relevance of these decision models to the present discussion is that they are not merely abstract "theories" of the firm, but actual decision-making devices. We can think of any such device as a simulation of the corresponding human decision-maker, in which the equations and other assumptions that enter into the formal decision-making procedure correspond to the decision premises—including the role prescription—of the decision-maker.

The actual application of such models to concrete business situations brings to light the information-processing tasks that are concealed in the assumptions of the more abstract classical models [65, pp. 51-52]:

(1) The models must be formulated so as to require for their application only data that are obtainable. If one of the penalties, for example, of holding too small inventories is the loss of sales, a decision model that proposes to determine optimal inventory levels must incorporate a procedure for putting a dollar value on this loss.

(2) The models must call only for practicable computations. For example, several proposals for applying linear programming to certain factory scheduling problems have been shown to be impracticable because, even with computers, the computation time is too great. The task of decision theory (whether normative or descriptive) is to find alternative techniques—probably only approximate—that demand much less computation.

(3) The models must not demand unobtainable forecast information. A procedure that would require a sales department to estimate the third moment of next month's sales distribution would not have wide application, as either description or prescription, to business decision-making.

These models, then, provide us with concrete examples of roles for a decision-maker described in terms of the premises he is expected to apply to the decision—the data and the rules of computation.

2. *Engineering Design*. Computers have been used for some years to carry out some of the analytic computations required in engineering design—computing the stresses, for example, in a proposed bridge

design. Within the past two years, ways have been found to program computers to carry out synthesis as well as analysis—to evolve the design itself.[30] A number of companies in the electrical industry now use computers to design electric motors, transformers, and generators, going from customer specifications to factory design without human intervention. The significance of this for our purpose here is that the synthesis programs appear to simulate rather closely the processes that had previously been used by college-trained engineers in the same design work. It has proved possible to write down the engineers' decision premises and inference processes in sufficient detail to produce workable computer programs.

3. *Human Problem Solving*. The management science and engineering design programs already provide examples of simulation of human decision-making by computer. It may be thought that, since in both instances the processes are highly arithmetical, these examples are relevant to only a very narrow range of human problem-solving activity. We generally think of a digital computer as a device which, if instructed in painful detail by its operator, can be induced to perform rather complicated and tedious arithmetical operations. More recent developments require us to revise these conceptions of the computer, for they enable it to carry out tasks that, if performed by humans, we would certainly call "thinking" and "learning."

Discovering the proof of a theorem of Euclid—a task we all remember from our high school geometry course—requires thinking and usually insight and imagination. A computer is now being programmed to perform this task (in a manner closely simulating the human geometer), and another computer has been successfully performing a highly similar task in symbolic logic for the past two years.[31] The latter computer is programmed to learn—that is to improve its performance on the basis of successful problem-solving experience—to use something akin to imagery or metaphor in planning its proofs, and to transfer some of its skills to other tasks—for example, solving trigonometric identities—involving completely distinct subject matter. These programs, it should be observed, do not involve the computer in rapid arithmetic—or any arithmetic for that matter. They are basically nonnumerical, involving the manipulation of all kinds of symbolic material, including words.

Still other computer programs have been written to enable a computer to play chess.[32] Not all of these programs, or those previously

[30] A nontechnical description of such a program will be found in [33].

[31] The program for proving theorems in logic is discussed in [51] and [52], Gelernter and Rochester's geometry program in [31].

[32] A survey of computer chess programs can be found in [54].

mentioned, are close simulations of the processes humans use. However, in some direct attempts to investigate the human processes by thinking-aloud techniques and to reproduce in computer programs the processes observed in human subjects, several striking simulations have been achieved.[33] These experiments have been described elsewhere and can't be reviewed here in detail.

4. *Business Games.* Business games, like those developed by the American Management Association, International Business Machines Corporation, and several universities, represent a parallel development.[34] In the business game, the decisions of the business firms are still made by the human players, but the economic environment of these firms, including their markets, are represented by computer programs that calculate the environment's responses to the actions of the players. As the games develop in detail and realism, their programs will represent more and more concrete descriptions of the decision processes of various economic actors—for example, consumers.

The games that have been developed so far are restricted to numerical magnitudes like prices and quantities of goods, and hence resemble the management science and engineering design programs more closely than they do those we have described under the heading of human problem solving. There is no reason, however, to expect this restriction to remain very long.

Implications for Economics

Apart from normative applications (e.g., substituting computers for humans in certain decision-making tasks) we are not interested so much in the detailed descriptions of roles as in broader questions: (1) What general characteristics do the roles of economic actors have? (2) How do roles come to be structured in the particular ways they do? (3) What bearing does this version of role theory have for macroeconomics and other large-scale social phenomena?

Characterizing Role Structure. Here we are concerned with generalizations about thought processes, particularly those generalizations that are relatively independent of the substantive content of the role. A classical example is Dewey's description of stages in the problem-solving process. Another example, of particular interest to economics, is the hypothesis we have already discussed at length: that economic man is a *satisficing* animal whose problem solving is based on search activity to meet certain aspiration levels rather than a *maximizing* animal whose problem solving involves finding the best alternatives in terms of specified criteria [64]. A third hypothesis is that operative goals (those

[33] Much of this work is still unpublished, but see [53] and [54].

[34] Two business games are described by Andlinger [1].

associated with an observable criterion of success, and relatively definite means of attainment) play a much larger part in governing choice than nonoperative goals (those lacking a concrete measure of success or a program for attainment) [45, p. 156].

Understanding How Roles Emerge. Within almost any single business firm, certain characteristic types of roles will be represented: selling roles, production roles, accounting roles, and so on [22]. Partly, this consistency may be explained in functional terms—that a model that views the firm as producing a product, selling it, and accounting for its assets and liabilities is an effective simplification of the real world, and provides the members of the organization with a workable frame of reference. Imitation within the culture provides an alternative explanation. It is exceedingly difficult to test hypotheses as to the origins and causal conditions for roles as universal in the society as these, but the underlying mechanisms could probably be explored effectively by the study of less common roles—safety director, quality control inspector, or the like—that are to be found in some firms, but not in all.

With our present definition of role, we can also speak meaningfully of the role of an entire business firm—of decision premises that underlie its basic policies. In a particular industry we find some firms that specialize in adapting the product to individual customer's specifications; others that specialize in product innovation. The common interest of economics and psychology includes not only the study of individual roles, but also the explanation of organizational roles of these sorts.

Tracing the Implications for Macroeconomics. If basic professional goals remain as they are, the interest of the psychologist and the economist in role theory will stem from somewhat different ultimate aims. The former will use various economic and organizational phenomena as data for the study of the structure and determinants of roles; the latter will be primarily interested in the implications of role theory for the model of economic man, and indirectly, for macroeconomics.

The first applications will be to those topics in economics where the assumption of static equilibrium is least tenable. Innovation, technological change, and economic development are examples of areas to which a good empirically tested theory of the processes of human adaptation and problem solving could make a major contribution. For instance, we know very little at present about how the rate of innovation depends on the amounts of resources allocated to various kinds of research and development activity [34]. Nor do we understand very well the nature of "know how," the costs of transferring technology from one firm or economy to another, or the effects of various kinds and amounts of education upon national product. These are diffi-

cult questions to answer from aggregative data and gross observation, with the result that our views have been formed more by arm-chair theorizing than by testing hypotheses with solid facts.

VII. *Conclusion*

In exploring the areas in which economics has common interests with the other behavioral sciences, we have been guided by the metaphor we elaborated in Section I. In simple, slow-moving situations, where the actor has a single, operational goal, the assumption of maximization relieves us of any need to construct a detailed picture of economic man or his processes of adaptation. As the complexity of the environment increases, or its speed of change, we need to know more and more about the mechanisms and processes that economic man uses to relate himself to that environment and achieve his goals.

How closely we wish to interweave economics with psychology depends, then, both on the range of questions we wish to answer and on our assessment of how far we may trust the assumptions of static equilibrium as approximations. In considerable part, the demand for a fuller picture of economic man has been coming from the profession of economics itself, as new areas of theory and application have emerged in which complexity and change are central facts. The revived interest in the theory of utility, and its application to choice under uncertainty, and to consumer saving and spending is one such area. The needs of normative macroeconomics and management science for a fuller theory of the firm have led to a number of attempts to understand the actual processes of making business decisions. In both these areas, notions of adaptive and satisficing behavior, drawn largely from psychology, are challenging sharply the classical picture of the maximizing entrepreneur.

The area of imperfect competition and oligopoly has been equally active, although the activity has thus far perhaps raised more problems than it has solved. On the positive side, it has revealed a community of interest among a variety of social scientists concerned with bargaining as a part of political and economic processes. Prediction of the future is another element common to many decision processes, and particularly important to explaining business cycle phenomena. Psychologists and economists have been applying a wide variety of approaches, empirical and theoretical, to the study of the formation of expectations. Surveys of consumer and business behavior, theories of statistical induction, stochastic learning theories, and theories of concept formation have all been converging on this problem area.

The very complexity that has made a theory of the decision-making process essential has made its construction exceedingly difficult. Most

approaches have been piecemeal—now focused on the criteria of choice, now on conflict of interest, now on the formation of expectations. It seemed almost utopian to suppose that we could put together a model of adaptive man that would compare in completeness with the simple model of classical economic man. The sketchiness and incompleteness of the newer proposals has been urged as a compelling reason for clinging to the older theories, however inadequate they are admitted to be.

The modern digital computer has changed the situation radically. It provides us with a tool of research—for formulating and testing theories—whose power is commensurate with the complexity of the phenomena we seek to understand. Although the use of computers to build theories of human behavior is very recent, it has already led to concrete results in the simulation of higher mental processes. As economics finds it more and more necessary to understand and explain disequilibrium as well as equilibrium, it will find an increasing use for this new tool and for communication with its sister sciences of psycology and sociology.

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INDIA AND CHINA: CONTRASTS IN DEVELOPMENT PERFORMANCE

By WILFRED MALENBAUM*

A decade has passed since the problems of economic growth in the poorer nations became a major foreign policy concern of the wealthy and powerful nations of the world. In the underdeveloped lands, the ten years reveal extensive planning activity as well as unprecedented inflows of technical and capital assistance on government account. Also during this period the imagination and efforts of many economists and other social scientists all over the world have turned to the task of uncovering the secret to the critical transition—the process by which stagnation may become growth, or progress at a slow rate may be accelerated. Yet very few countries have succeeded in making this transition during the decade. In Asia, where live a large part of the world's population and an even larger part of its poor people, India and mainland China alone offer some prospect of such achievement in the near future.

The relative progress in the development of these two countries is of great significance. There were strong parallels in their preplan structure and strong contrasts between China's totalitarian and India's democratic programs [18, pp. 1-24]. Their performance relative to one another may influence the programs adopted by other, now less advanced, countries. It will certainly bear upon Soviet and United States foreign policies. Furthermore, the record of the course of development in these two lands provides a unique opportunity for examining the process of development as such. What are the essential economic ingredients? Can they be used with equal effectiveness in democratic and communist societies?

I. *Comparative Performance*

India is now in the last half of its second five year plan. China initiated its second plan on January 1, 1958. Records can actually be

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compared over the nine-year period ending in early 1959. Of course, records are available "more or less." India has continued to publish extensively on its economy and its problems. Since mid-1955, there has been a great expansion in the information available for mainland China in official communist publications and in an expanding flow of press and visiting mission reports.

In some major respects, comparison of the development record is hampered more by lack of data from "open" India than from "closed" China. It is more difficult to estimate national savings or investment when a private sector plays an important role in these basic economic activities than when they are more nearly the prerogative of government. Furthermore, officialdom in India has not yet adapted its statistical services to the requirements of a national development program. Thus, even the official record of the first five year plan does not present investment data for each of the years 1951-1956. There is no explanation as to how the rough figures for total investment over the five years were obtained, nor any indication of their sectoral allocation. It is not clear just how such basic magnitudes as total investment in sectors of the economy play their role in planning or in postauditing the plans. In contrast the Chinese, less encumbered, it is true, with the traditional statistics of normal times, focus pointedly on "accumulation," or on "capital constructions." They leave no doubt that these are key magnitudes for their own needs, in planning and in actual development operations. In these circumstances, nonofficial and even personal estimates play a significant role in the Indian data, as well as in the Chinese. On the other hand, it is possible to discuss such estimates with Indian officials, to study basic underlying data, and to observe. For China, much of the data, even official data, must remain simply numbers to most students. The data can be "tested" only through checks of internal consistency, or logical relationship to some past figures.

A. *Gross National Product and Gross Investment*

The aggregate figures used in the present analysis are given in Table 1.

1. *Relative rates of change.* India achieved an annual rate of growth of real income of almost 3.5 per cent in the period from April 1, 1950 through March 31, 1959. Over essentially the same period, the Chinese growth rate was at least three times as great. China recorded impressive gains in the preplan years when expanding meant primarily reactivating and rebuilding; large increases in output were the results of greater use of existing plant rather than of new investment. It was not until 1952/53 that China regained past levels of aggregate real

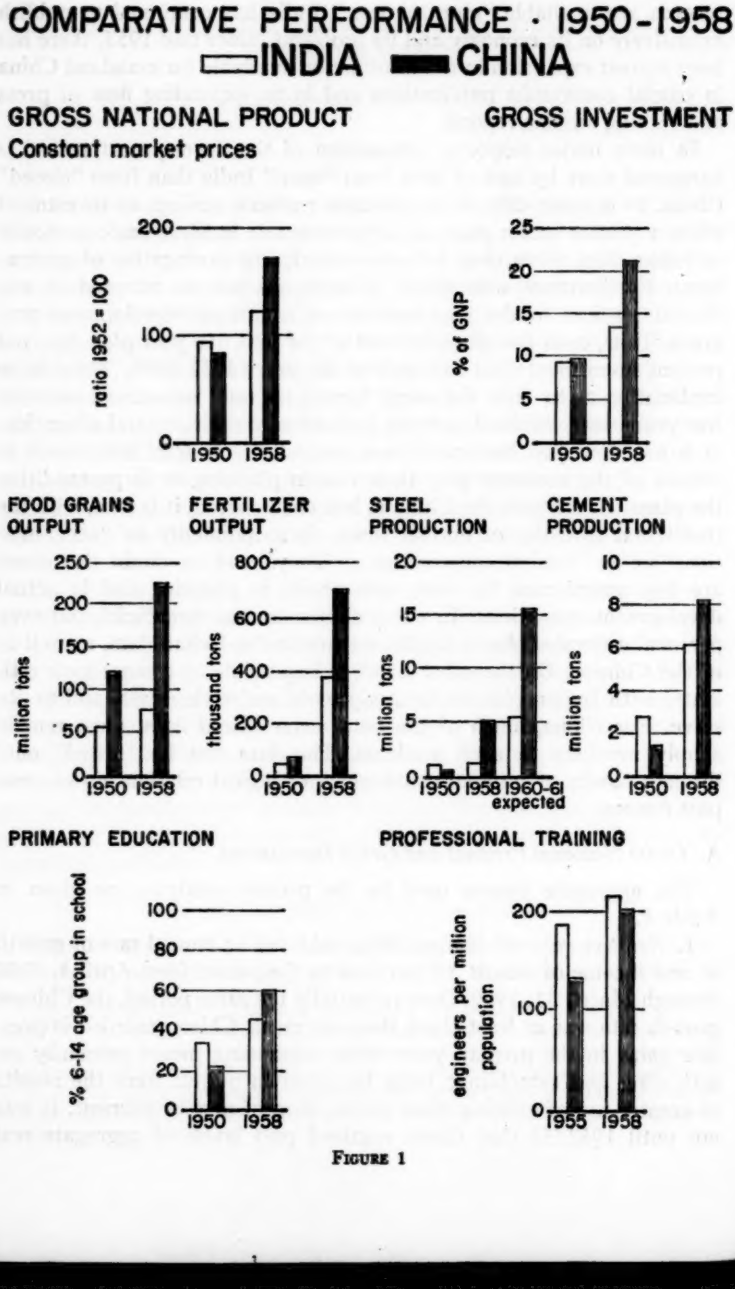
COMPARATIVE PERFORMANCE · 1950-1958
INDIA
CHINA
GROSS NATIONAL PRODUCT
Constant market prices
ratio 1952 = 100
200
100
0
1950
1958
GROSS INVESTMENT
% of GNP
25
20
15
10
5
0
1950
1958
FOOD GRAINS OUTPUT
million tons
250
200
150
100
50
0
1950
1958
FERTILIZER OUTPUT
thousand tons
800
600
400
200
0
1950
1958
STEEL PRODUCTION
million tons
20
15
10
5
0
1950
1958
1960-61 expected
CEMENT PRODUCTION
million tons
10
8
6
4
2
0
1950
1958
PRIMARY EDUCATION
% 6-14 age group in school
100
80
60
40
20
0
1950
1958
PROFESSIONAL TRAINING
engineers per million population
200
100
0
1955
1958

FIGURE 1

TABLE 1—GROSS NATIONAL PRODUCT AND GROSS INVESTMENT
(At Constant Market Prices)
A. India

Year[a]	Gross National Product: Billions of Rupees (1952 Prices)	Gross National Product: Ratios (1952=100)	Gross Investment: Billions of Rupees (1952 Prices)	Gross Investment: Ratios (1952=100)	Gross Investment: Per Cent of GNP
1950	101	93.4	9.4	88.7	9.3
1951	103.7	95.9	9.9	93.4	9.5
1952	108.1	100	10.6	100	9.8
1953	114.4	105.8	11.1	104.7	9.7
1954	117.8	109	13.4	126.4	11.4
1955	120.1	111.1	14.5	136.8	12.1
1956	126.1	116.7	16.7	157.5	13.2
1957[b]	126.8	117.3	17.2	162.3	13.6
1958[c]	130	120.3	17.5	165.1	13.5

B. China

Year[a]	Gross National Product: Billions of Yuan (1952 Prices)	Gross National Product: Ratios (1952=100)	Gross Investment: Billions of Yuan (1952 Prices)	Gross Investment: Ratios (1952=100)	Gross Investment: Per Cent of GNP
1950	55.02	81.1	5.31	52.6	9.7
1951	62.85	92.6	7.39	73.2	11.8
1952	67.86	100	10.09	100	14.9
1953	77.06	113.6	12.94	128.2	16.8
1954	81.92	120.7	15.65	155.1	19.1
1955	85.41	125.9	15.38	152.4	18
1956	97.21	143.2	18.62	184.5	19.2
1957	102.42	150.9	24.47	242.5	23.9
1958[c]	117.50	173.2	25.85	256.2	22

[a] For India, accounting year begins April 1; for China, January 1.

[b] Estimates constructed from preliminary official component data.

[c] Orders of magnitude based upon general reports from the two countries. For China, the 1958 figures are conservative; it now appears certain that later estimates will be higher—by amounts which cannot yet be indicated.

Sources and Nature of Estimates:

1. All data for India are my personal estimates based upon official materials available through 1956/57 and to a lesser extent for 1957/58 and 1958/59. The government of India (GOI) does prepare estimates of national income both at factor cost and market price level; these are published in an annual "white paper" in March [6]. Government has not yet released official annual estimates of capital formation. Where the GOI does make use of investment figures these are given on a net basis (generally for groups of years together), for monetized investment only, and exclusive of changes in stocks [13, pp. 9-10], [14, pp. 8-11]. Finally, GOI estimates in constant prices are based on the year 1948/49.

Where a large proportion of national product is generated without benefit of financial transactions, as is true for much of agriculture and rural activity in India (and China) nonmonetized investment is of great importance. Indeed, in the very nature of the rural economies, it becomes difficult if not impossible to categorize any meaningful component

(Continued on page 288)

Footnote to Table 1—Continued

of private and sometimes, even public rural investment as the monetized component. Similarly, the accounting devices available when most output is produced by entrepreneurs who are generally illiterate raise doubts about the depreciation adjustment for the private sector, especially in small enterprise. Moreover, "replacement" for depreciated capital takes on a net aspect where the new equipment is modern and the existing machinery long outdated—the frequent situation in these economies. Official data provide estimates for depreciation on government account only. However, the GOI has released "unofficial" estimates on a gross basis and inclusive of transactions made on a barter basis [8, pp. 154-59]. These data and subsequent materials from the same source were used in deriving the estimates for Table 1.

Preliminary unofficial estimates for changes in working stock suggest that these expanded by only 1.4 billion rupees over the first plan. The year-to-year pattern of changes indicates wide annual variations rather than any persistent movement. In view of the partial nature of these data, they could not be incorporated into Table 1. Finally, all data available only in current prices, or in constant 1948/49 prices, were shifted to a 1952/53 price basis by the use of official series, with separate price indexes applied for the consumption and investment components of the gross product. (Thus the data of Table 1 do not reflect the post-1952 tendency for the unit costs of investment to increase relative to other costs.)

All these adjustments serve to raise the general level of both the national product and investment series for India in the table above those usually presented; year-to-year fluctuations are less affected, especially for the national product series.

2. Data, 1950-57, are from W. W. Hollister [5, pp. 132-33]. These were constructed from budget and retail sales data, and augmented by direct estimates for farm consumption, private investment, various consumer services, and inventory changes [5, pp. xxii-iv]. Except for the inclusion of changes in working stocks, the data are analytically identical with those presented for India. I have added the 1958 figures. They reflect about as large an increase in real product as any actually achieved in the period since 1950. The investment ratio for 1958 (22 per cent) has some basis in official source materials. (Actual developments of 1958 assure that gross real product will be appreciably above that in the table.)

The gross national product figures of Table 1 are generally higher than those derived directly from official Chinese sources; also they increase at a somewhat slower rate than do the more official estimates. On the other hand, Table 1 estimates for gross investment tend to be below official estimates. (The most convenient source for the official data is in the translations prepared by the American Consulate in Hong Kong. A recent survey [26] gives basic data for national income, and its separation into consumption and investment. These are reported in current prices.)

Official estimates of output are formulated with a Marxist concept of value: they exclude certain transport and service earnings which are included in Western (and Indian) national income figures—and in the Hollister data. The differences in the rate of increase in the two series can be attributed at least in part to important underestimates in official output figures in agriculture, 1950-53, thus exaggerating the upward trend in total output over the plan years [5, pp. 17-23].

With respect to investment, the present estimates have been built up by major sectors, on the basis of all the Chinese material available. For 1953-57 these components and their total are consistent with the complete account of investment expenditures presented by the Chinese in their 1956 publication on the first five year plan [1, pp. 27-37]. This governmental source gives 89.2 billion yuan as the five-year total, on both public and private account, for various kinds of investment and for depreciation as well as for expenditures for the "maintenance of schools" and for "operating expenses of urban public utilities"—items appropriately considered current rather than capital outlays. Nonetheless, this comprehensive total is below the five-year total (92 billion yuan) of the official investment figures—and these are presented as net investment in fixed and working capital. The offi-

(Continued at bottom of page 289)

product. In India, on the other hand, the expansion has meant new levels of output more or less since 1950. In addition to comparisons over the entire period therefore, it is appropriate to contrast rates of growth for the respective first-plan periods, from 1951 in India and 1953 in China. Table 1 shows a 19 per cent growth in India's gross product during 1951-56, the first-plan period, in contrast to China's 51 per cent for 1953-57. There has been a higher annual rate of population growth in China than in India—perhaps 2 to 2.2 per cent as against India's 1.4 to 1.7 per cent in these years; increases in per capita gross income show a somewhat narrower margin for China over India than do increases in total income.

Gross investment ratios were close to the same level in 1950; thereafter they increased about three times as fast in China. Moreover, given the greater expansion in Chinese product, these ratios mean that in 1957 and 1958 the real level of gross investment in China was about five times what it was in 1950; in India, it was not quite twice the 1950 level. Can these differences be in any way attributed to differences in foreign capital inflows over these years? Apparently 3.1 per cent of China's gross investment in the years through 1957[1] (and some 2.1 per cent during the first plan) were offset by a net import surplus. The comparable figures are 8.2 per cent (and 2.3 per cent) for India. If anything, therefore, China financed more of its investment program on the basis of domestic savings from current income. However, this is quite consistent with the fact that China obtained relatively more assistance from other countries. The net import surpluses in China were financed essentially by loans from other communist countries and especially the Soviet Union. In India, on the other hand, a reduction in foreign ex-

[1] Data for 1958 were not included in this comparison, which is thus confined to the 8 years ending in 1957. As indicated in the next paragraph of the text, the preliminary evidence is that the contrast will be strengthened further when firm 1958 figures are available.

cial investment series appears to be the result of applying a net investment ratio to estimates of national income. No sectoral data are given, and the total net investment is consistently larger than other investment data—for "economic expenditures" and for "capital construction expenditures"—used by the Chinese for budgetary purposes. Components of these latter are given in detail and over time; they include working capital, research and development expenditures, and outlays for repair and replacement as well as for new capital. Because of the lack of component data in the official series and because its totals of *net* investment are consistently above careful estimates—including some by the Chinese themselves—for gross investment, the investment series of Table 1 are considered to be more meaningful than those officially presented.

It should also be noted that, in converting estimates in current prices to a 1952 basis, separate price indexes were applied to gross investment. This retains the 1952 price relationship of capital to other goods. (There is some tendency in China for the prices of other goods to increase relative to capital goods in the years after 1952—the opposite of that noted above for India. It was therefore important to eliminate the effect of this divergent movement from the original data.)

change reserves made possible some 60 per cent of the import surplus, with only the residual amount dependent upon foreign grants and loans.

China seems to have achieved some balance in its foreign accounts. For 1957, for example, the international account shows a small export surplus such as would be consistent with the need to repay past borrowings [5, pp. 127-29, 132-33].[2] India's foreign-trade deficit was larger in 1956/57 and again in 1957/58 than in any other year considered here. The need for an import surplus may long continue. The differing experiences on foreign account mean that gross domestic savings ratios have expanded at an even more rapid rate in China, relative to India, than is the case for the gross investment series of Table 1. This is also borne out by comparing the increase in domestic savings as domestic product expands. Over the entire period 1950-58, China allocated at least 40 per cent of the expansion in gross output to investment; for the first plan period, 1953-57, it was 44 per cent. Over the same two time intervals in India, the marginal propensity to save was almost 20 per cent, although for the first plan itself it was 38.5 per cent.[3] Even in democratic India, the propensity to consume was significantly smaller than is usually assumed to be the case in poor agricultural countries.

2. *Absolute comparisons.* China's higher rates of domestic savings obviously mean that India can allocate to consumption a higher ratio of its domestic product. Furthermore, current expenditures by government —part of consumption—run at appreciably greater relative levels in China than in India: the respective ratios in 1952 were 10 per cent and 7 per cent. During many of the years 1950-58, not more than 75 per cent of national product thus became available for household consumption in China; in India the percentage has yet to fall as low as 80 per cent and is usually about 85 per cent. What are the absolute levels

[2] On June 29, 1957, the Chinese Minister of Finance stated "—we may say that we are now in a better position to rely on our own accumulation to carry on national construction . . ." (quoted by P. C. Mahalanobis [15]). Mahalanobis, director of the Indian Statistical Institute and the person responsible for the statistical basis for Indian planning, attributed the favorable performance in China to a concentration upon the production of "basic industries (heavy machinery, heavy electricals, machine tools, steel, fertilizers, trucks, etc.)" to which Russian aid was in considerable measure directed. India's foreign exchange difficulties arise, he argues, from the failure to pursue a similar source. But a sufficient interpretation must run in terms of the growth of output and savings. Thus, assuming that Russia and Czechoslovakia, for example, were prepared to continue to export capital goods, China's ability to invest might have been at least as great if the export surplus of grains had been expanded at the expense of a growth in basic industries. Which is better depends upon the relative costs incurred in acquiring the same amount of capital goods by the two methods.

[3] No inconsistency need exist between this high figure for 1951-56 and the ratios for 1951-58 and 1953-58. The savings series fluctuates: its peak is in 1955/56. These computations are crude approximations to the marginal propensities at best, since they have been computed directly from the figures in constant prices.

of income to which these very different consumption ratios are applied? Can China drain larger shares of national product away from consumption because the Chinese have higher levels of living than do Indians?

Some absolute comparison of output in the two countries is possible.[4] India and China have had similar enough economies so that analytical problems in making the comparisons are relatively small. On the other hand, some of the data needed are not adequately reported, especially the prices. The data available permit the following conclusions:[5]

a. Per capita agricultural product was about 15 per cent higher in China than in India in 1952. This is consistent with a higher differential in favor of China for food grains alone, since other foods play a larger role in India and since China usually exports and India imports grains.

b. For the rest of the economy, per capita output was higher in India in that year. This conclusion involves some judgment, since data did not permit a complete comparison for all or nearly all other goods and services. For commercial fuel and power the per capita advantage seemed to be with China; in heavy industry, India was ahead by at least a 20 per cent margin; comparisons for other items ranged between these limits. Quantitatively, nonagricultural product per person might have been some 10 per cent higher in India.

c. Per capita incomes in the two countries in 1952 were essentially of the same order of magnitude—about Rs. 260 or 130 yuan. This is concluded simply from the preceding two statements. Agricultural income provided more than half the total product in China in 1952; it constituted somewhat less than half in India. Despite very large differences in favor of India in the national income estimates generally made, it seems more likely that the actual incomes were about the same in 1952.[6]

At this relatively early stage of the development effort in both countries, China was investing some 50 per cent more than was India out of essentially the same real product.[7] After allowing for the different levels of government consumption, it appears that the average level of household consumption in China was actually 10 per cent to 15 per cent below that in India. As Table 1 suggests, this situation changed rapidly. Sometime in 1955 or 1956—despite China's larger allocations to investment and to other governmental uses, and despite its more rapid

[4] Gilbert and Kravis [3], for the techniques to be employed.

[5] I am indebted to George Perry and Delmar Underwood, graduate students at Massachusetts Institute of Technology, for computations upon which these were in part based.

[6] Comparisons in which estimates in national currencies were converted directly into U. S. dollars at the going rates of exchange place per capita income in China in 1946 at $23, and in India at $43 [24, p. 9].

[7] In 1952, net import surplus financed 9 per cent of China's gross investment. India had a net export surplus equal to about 5 per cent of its gross investment.

rate of population growth—the per capita levels of household consumption began to forge ahead of the levels prevailing in India.

B. *Levels of Physical Output*

1. *Agriculture.* Table 2 presents series for the all-important food grains, as well as for sugar cane and the major fibers, cotton and jute. Up to 1958—for which results are still uncertain—the data indicate a larger expansion in food grains in China than in India. However, India's performance during its first plan was more impressive than was China's in 1953-57—a 30 per cent increase compared with a 22 per cent increase. On a per capita basis, domestically produced food grains in India increased very rapidly from 335 pounds in 1950/51 to 444 pounds in 1953/54. The level in this latter year of very favorable weather was not again attained in the four following years. Indeed output per capita in 1957/58 has fallen below 400 pounds. In China, there was an early increase from 500 pounds to 600 pounds; favorable harvests in 1954/55 then boosted the per capita figure to 645 pounds. The two years through 1957/58 have seen somewhat higher levels, close to 660 pounds per capita. Given the importance of weather in the year-

TABLE 2—AGRICULTURAL OUTPUT: MAJOR COMMODITIES

Year[a]	Food Grains[d] (Million tons)		Cotton (Thousand tons)		Jute[e] (Thousand tons)		Sugar Cane[f] (Million tons)	
	India	China	India	China	India	China	India	China
1950	53.5	122.72	509	681.5	586	77.6	56.2	3.1
1951	55.06	132.92	548	1014.3	835	245.7	60.7	4.6
1952	62.45	151.96	559	1283.1	820	300.7	50.2	7
1953	74.08	154.42	690	1156.2	552	135.7	44.2	7.1
1954	69.76	157.9	740	1048.1	523	134.5	56.9	8.5
1955	69.93	174.81	700	1494.4	749	252.7	59.3	8
1956	73.2	182.5	830	1445	768	258	67	8.7
1957[b]	67.1	185	835	1640	730	305	64	10.2
1958[c]	72.5	225	825	2500	750	325	65	13

[a] Crop year beginning in:
[b] Official estimates for 1957/58 subject to final revision.
[c] Preliminary. Estimates given are conservative; final figures may turn out to be substantially higher.
[d] Excludes pulses but includes potatoes; rice in terms of paddy. (Data for India are usually given in terms of clean rice; on the average in India, 3 pounds of paddy yield 2 pounds of rice.
[e] Includes hemp for China.
[f] Excludes sugar beets which are important in China.

Sources:
India: 1950–54, (10, pp. 92–102)
1955–58 (9 current).
China: 1950–55 (10, pp. 92–102).
1956–58 (25 Despatch No. 884, April 30, 1958).

to-year pattern of output in both countries, however, it is perhaps more significant that only the Chinese data provide some evidence of a persistent upward trend. While this might suggest a greater measure of success in overcoming the natural and human deterrents to expanding production, the record is scarcely definitive.

Indian agricultural statistics are consistent with the claim that the potential for food-grains output has expanded in the course of the first-plan years to a new level, some 20-25 per cent above preplan production. At least half of this might be attributed to additional acreage, mostly the result of expansion in the area irrigated. The remainder could be due to the effects upon yields per acre of a number of developments, including more fertilization, better seed, and improved farm practices generally. However, these underlying factors would be expected to exert their influence gradually, while the output data suggest a shift to a new level during 1952 and 1953, without systematic expansion thereafter. Even the rather favorable estimate for the 1958/59 crop can be attributed in part to climatic conditions; in any event, grain output of 72.5 million tons does not necessarily mean that an upward trend in output has been resumed.[8]

Although food-grains output in China in 1957 is officially reported to exceed the five-year-plan targets by 1.9 per cent, Chinese sources have made clear that performance in this sector has been disappointing. And some observers abroad argue that the official record has actually exaggerated the true achievement.[9] But even so, limited progress is indicated through 1957. Of the 20 per cent expansion during the plan, about 75 per cent represented increases in yields per acre. To some extent this was also true in the preplan rehabilitation period, although acreage expansion was then relatively more important. Systematic change—the persistent growth in ouput, however small, and the consistency of the contributory factors—probably constitutes the most significant aspect of Chinese development in this area.

Other agricultural production in China also reflects the major re-

[8] A principal conclusion of the first round of India's National Sample Survey was that ". . . official statistics [of food-grains production in 1948/49 and 1949/50] seem to be under-estimates by something between roughly 20-25 per cent . . ." [7, p. 26]. While the official statistics do reflect a subsequent expansion of this order of magnitude, the NSS view is not universally accepted in India. See for example Dandekar [2, pp. 153-65].

[9] The American Consul at Hong Kong indicated that 1957 output levels were "not unreasonable but the means of derivation warn against treating them as solid" [24, Despatch No. 884]. Foreign experts claim that the data for 1950-1953 are too low and thus overstate the degree of improvement [5, pp. 17-23]. One careful study argues that the official figures, whatever the actual expansion they reveal since 1949, are still below the output levels of the early 'thirties. The 1957 target would only achieve that earlier production level—due to an underestimate (by about one-third) of the actual prewar output [22, *passim*].

habilitation efforts of the years through 1952/53. Thereafter it is only in cotton, a key product for Chinese industry, where there is evidence of a major drive for expanded output. In India products other than grain reveal about the same proportionate output increase over the entire period through 1957/58 as do the food grains. On the whole, the record in agriculture suggests a more impressive performance for China in the food grains; for India in other food products and industrial raw materials.

These offsetting tendencies are reflected in the indexes of agricultural output available for the two countries. From 1950 through 1957, aggregate output in agriculture rose by some 25 to 30 per cent in China and 15 to 20 per cent in India. This is a much smaller difference than exists for the food grains alone. Agricultural production has increased at a lower rate in China than has aggregate production; income from agriculture has become of lesser importance in the total product. For India, agriculture and the rest of the economy seem to have kept more nearly in line.[10]

The agricultural effort in China has been closely directed by government. While there was much talk of broadening and improving diets, the focus has been on the food grains, in order to derive maximum energy output per unit of expenditure for agricultural expansion. Even in adverse-weather years the Chinese did succeed in expanding grain output by about as much as population. The picture in India has been quite different. The degree of government direction was relatively small. Profitability considerations governed with respect to commercial production. The bulk of the output, especially grains, is produced for local or even for producer use, and the underlying motivations here are less readily characterized. Certainly the programs of government—for expanding the acreage under irrigation, for improving methods of cultivation including the greater application of commercial fertilizers—had a smaller influence than the development plans promised. Weather remained the predominant factor and accounts for the largest increases in production observed over past years.

The scope for improvement in agriculture remains large, and particularly in India [18, pp. 7-8, 11-12]. Thus rice, the preferred food grain, accounts for about half the grain output in both countries. Yet the yield per acre in China in 1957 was about half that in Japan; it is generally about twice that in India. Officialdom in both countries has recognized the need to devote much greater effort to this sector. Thus, over the past two years, Indian leadership has increasingly questioned whether a basis for systematic expansion of food-grain output has in

[10] See below, p. 301.

fact been established in India. Today this is the major problem on the Indian development scene, and new programs for agricultural progress are receiving ever greater priority.[11] In China, new efforts initiated in 1957 began to manifest themselves early in 1958. Already the rapid extension of small-scale irrigation, the increased rates of fertilization, and much more intensive cultivation bid fair to assure record levels of crop yields for 1958/59, and this despite relatively unfavorable weather in many parts of important agricultural regions.[12]

2. *Industry*. For major industrial products (Table 3) the comparative records are more straightforward. Aggregate industrial output in the modern sector has made much greater progress in China. Both indexes give some evidence of a slackening of the rates of growth of big industry during 1957, although current reports from China suggest that the revised figure for 1958 will testify to a resumption of a pronounced upward trend. In interpreting the series, it should be noted that 1950 was still a year when expansion was primarily the result of reactivation and rehabilitation of existing plant, and this was true to a greater extent in China than in India. Also the Chinese indexes suffer from considerable double counting given Marxist procedures for adjusting for intermediate goods in production. Fortunately, available data permit straightforward comparison for physical output of more or less identical products.

For every commodity in Table 3 Chinese rates of increase have exceeded India's by sizable amounts. In some cases—steel, cement, electrical power, textiles—Chinese output and capacity were smaller in 1950 than India's. In fertilizer and coal, larger absolute production in China was about the same, or even smaller on a per capita basis. In every case, however, production levels for these major commodities are now above those of India. Through 1957 India did retain some advantage in cement and fertilizer on a per capita basis, but it is probable that such margins are now disappearing.

On the basis of the less comparable information available for consumer goods, Chinese performance is not nearly so spectacular. As in

[11] India's National Development Council, in January 1958 and again in May, made pointed reference to the unevenness of past results and to the gap between expenditures and performance. Striking were the observations on the failure to make use of the irrigation facilities already constructed. For the outlines of the crash program adopted for the current crop, see [11, pp. 39-49] [12, pp. 13-15].

[12] Reports stress the "big leap forward" in crop yields. Thus the Chinese claim that the major early rice crops already promise a 50 per cent increase over the 1957 yields per acre. Total food-grain output is officially projected at 300 million tons, almost 65 per cent above last year's crop. [25, Despatch No. 995, June 13, 1958; No. 364, November 3, 1958]. While this may reflect early optimism, a record increase can be expected. As was indicated earlier (p. 293), there is evidence that first-plan levels of grain output are well below prewar figures of 200 to 220 million tons [22, pp. 11-20].

TABLE 3—INDUSTRIAL OUTPUT: MAJOR COMMODITIES

Year[a]	Aggregate Production 1952=100		Steel (million tons)		Cement (million tons)		Electric Power (million kwh)		Coal (million tons)		Fertilizer (Ammonium Sulphate) (thousand tons)		Textiles (million yards)	
	India	China	India	China	India	China	India	China	India	China	India	China	India	China
1950	85	37	1.01	.40	2.68	1.41	5,112	4,580	32.5	40.9	47.3	75	3,650	2,940
1951	96.5	76	1.08	.79	3.19	2.48	5,856	5,790	34.3	50.8	52.7	129	4,065	3,570
1952	100	100	1.10	1.19	3.54	2.86	6,192	7,261	36.1	63.5	220.3	181	4,600	4,700
1953	102	129	1.02	1.56	3.78	3.88	6,708	9,165	35.8	66.6	319.6	226	4,875	5,900
1954	109	152	1.25	1.95	4.41	4.60	7,500	11,001	36.8	79.9	340.2	298	5,000	6,250
1955	118	166	1.26	2.51	4.48	4.50	8,496	12,278	38.3	93.6	393.1	324	5,090	5,330
1956	128	226	1.34	3.88	4.93	6.42	9,636	16,588	39.4	105.9	389	446	5,310	6,500
1957	133	244	1.35	4.26	5.58	6.69	10,836	19,025	43.5	130	383	535	5,315	5,825
1958[b]	137	288	1.27	5	6.07	8.18	12,198	23,000	44.8	165	381.6	700	4,925	6,250

[a] Calendar years.

[b] Preliminary: for India, based on monthly reports through September 1958 [21]; for China, based on latest available U. S. government reports [25]. Estimates given are conservative; final figures may turn out to be substantially higher.

Sources:

India: Official Series [21].

China: Based on figures in [25]; prior to 1952, estimated from employment and other data.

the case of textiles, there is evidence of a deliberate limitation of industrial production for consumers whenever such output would curtail the supplies of power, transport, management and other scarce inputs for the hard producer-goods industries. On the whole, output of consumer goods by modern industry seems to have expanded more in India during 1950-57, and from an originally higher level.

Details as to China's handicraft and cottage industries under the Communists are not at hand, but there appears to be little parallel to the direct emphasis given this sector, so important in consumer-goods output, by the government of India. On the other hand, the Chinese have given much attention to the expansion of modern small-scale industry. This sector constitutes an important adjunct of big industry in a labor-rich country, and China has deliberately furthered an increase in output from a wide variety of small industries—including the production and processing of chemicals and pig iron. Achievement here is far beyond what has yet been accomplished, or even projected, by India.

3. *Education.* In regard to education and professional training, India seems to have started its development program with a considerable advantage. There were some 22 million children in school in 1950, almost one-third of those in the 6-14 age group. Today there may be 37 million, 45 per cent of those in this age group [14, pp. 501-4] [11, pp. 91-93]. China had a lower percentage in 1950 (22.5 per cent) but is reported to have almost 60 per cent of children aged 6-14 in school today [1, pp. 201-3] [25]. At the other end of the educational process, China in 1955 was training annually 30.9 engineers and 11.2 medical doctors per million persons in its population. Comparable figures for India were 18.4 and 8.1 respectively. Continuation of these rates would, within ten to twenty years, reverse the more advanced position which India had in these professional fields in 1955 [15, p. 5].

Additional activities might be mentioned, although the major areas have been considered. Thus, cinema attendance and passenger travel have increased considerably in India over the past decade or so—probably much more than in China and again from an initially higher level. It is not possible to extend such physical comparisons to encompass the entire national product. Nor will this ever reflect differences of a qualitative sort—the variety of food and other consumer goods in India, the opportunity to select goods and services. On the whole, however, the Chinese margin of 1952 in per capita agricultural production does seem to have been maintained—and even widened over the years to 1958. With respect to big industry, India lost ground relatively—the result of a large expansion in China which made its modern industry sector a much more important part of total product than is

India's. However, the actual availability of nonfood consumer goods to the Indian citizen seems to have increased more than in China. Given the major role of grain in consumption and of food in the levels of living in the two countries, these general statements certainly do not impair the plausibility of our earlier conclusion on the relative growth of national output in India and China.

The record of comparative performance thus reveals that China has taken greater strides in investment, and this on the basis of greater reliance upon domestic savings. Gross output per person actually increased more than twice as fast as India's. Indeed Chinese consumption (governmental and personal) per capita in 1957 was about 20 per cent above the 1952 figure; the comparable increase in India was 8 per cent in 1955/56 over 1950/51. The present analysis thus indicates economic developments overwhelmingly favorable to the Chinese effort, both with respect to actual performance and to potential for further growth.

II. *Reasons Underlying the Contrast in Performance*

What explains the different results? One characteristic of the data which warrants special attention is the relationship between investment and total output. Table 1 yields the following ratios of gross investment to increase in gross product over the indicated time intervals:

	India	*China*
1950-1957	4	2.2
First-plan	3.1	2.5
Pre-first-plan	6.7	1.4
Post-first-plan	5.1	1.7

For the period as a whole, China seems to have generated a unit of gross-income flows with little more than half the gross investment that was applied in India.[13] Only for the plan periods proper are the cal-

[13] So striking are these differences that the computations warrant a few further observations. The underlying statistics of product and investment were presented as comparable sets of estimates (see notes to Table 1, above). In fact, the major departure arose from our inability to include changes in working stocks in India's gross investment; on this account the ratios above *understate* the difference between India and China.

Official Chinese figures for depreciation in the economy as a whole do tend to be on the low side. But these estimates do not play a direct role in the figures of gross investment used here. In any case, low depreciation levels in the official statistics mean high estimates of accumulation (net investment) rather than low estimates of gross investment. Another point is that, in communist countries generally, prices of capital goods, and indeed unit costs of capital formation, tend to be low relative to other prices. This might in itself provide lower capital-output ratios than prevail in a free market economy. However, while it was not possible to make a direct comparison of the pertinent price relationships in India and China in 1952, the deflating procedures used here maintained in each country the price relationships of that year. The different relative movements over the years should thus not affect the capital-output calculations.

With regard to the ratios themselves, it is true that they fluctuate markedly from year to year in each country; most of the specific figures above would thus be different were

culations for the two countries in reasonable line—with India's about one-quarter above the figure for China. The record thus indicates greater "efficiency" in China in converting a given amount of gross investment into additional capacity or at least product.[14]

This phenomenon is magnified by the fact that the Chinese have been able to increase their allocations of current product to investment at a more rapid rate. Rough calculations suggest that for the period as a whole, 55 per cent of the difference in the rate of growth in the two countries can be attributed to the greater efficiency with which the Chinese apply investment; 45 per cent is thus attributable to the more rapid rate of expansion in investment.

A. *Different Allocation of Investment*

The different results must be traceable to allocations of investment, and to the specific forms in which investment is actually made. Thus, scale of plant may reduce capital costs per unit of product; investment costs will be low if outlays for labor in construction, for example, exclude or undervalue contributed or forced labor, and the like. What evidence is there for differences betwen the development effort in India and China in these regards?

While data will not permit ready comparison of the allocation patterns in all years, it is possible to separate out for both countries the investment in two broad groupings: agriculture, including irrigation, water conservancy, community development and some simple handicrafts close to agriculture such as rice polishing; and industry, which includes modern industry and power. Table 4 gives these allocations on a percentage basis for groups of years.

Over the period, investment has become relatively less important in agriculture and more important in industry, as would be expected. Both movements have been more marked in China. More significant, however, are two other comparisons reflected by the table. Contrary to the impression which prevails generally,[15] a large percentage of total invest-

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the time coverage altered somewhat. Yet, there are no groups of years which would give reverse results, or indeed very different relative results, for the two countries. Thus, were rough adjustments made for the special influences which weather had upon output in some years in each country, the relative ratios would reveal a wider margin in favor of China than is shown for the two plan periods, for example. Similarly, inclusion of the preliminary data for 1958 does not appreciably alter any of the calculations. Thus, differences observed are not attributable to the particular groups of years selected for the comparisons.

[14] In characterizing the conversion as more "efficient" I mean only that less capital is used for the same volume of output, presumably with the same lag.

[15] My earlier observations [18, pp. 13-14] were also in this vein. Actually, agricultural investment was planned at less than 10 per cent of the public program but much larger amounts were to be invested by the peasants themselves ". . . in addition the development

TABLE 4—ALLOCATION OF GROSS INVESTMENT
(Per cent)

	Preplan		First Plan				Second Plan
			First 3 Years		Last 2 Years		
	India (1950)	China (1950–52)	India (1951–53)	China (1953–55)	India (1954–55)	China (1956–57)	India (1956–57)
Agriculture	28	32.7	26.5	27.6	26.5	27	24.5
Industry (including power)	23.4	36.6	25.6	35.5	24.3	45	29.2
Other sectors	48.6	30.7	47.9	36.9	49.2	28	46.3

Sources:
India: My estimates based on provisional data released by government [9, pp. 154–59].
China: Estimates based primarily on [5] [25] [1].

ment has been allocated to the agricultural sectors in China; throughout the period, this percentage has exceeded India's. In addition, there has been a marked difference in the emphasis given by China to physical product as against services. Thus, despite the superiority of India's road and railway network in the preplan years, India in the last few years seems to have allocated almost as large a proportion of total investment to transport and communication as China allocated to these plus social services, trade and finance, education, health and the like.

The striking contrasts notwithstanding, Table 4 itself throws little light on the problem of "efficiency" of investment. China puts more into agriculture, which has frequently turned out to be a sector where relatively large returns follow from a unit of new capital—at least in underdeveloped areas where yields per acre are very low initially. On the other hand, the still larger Chinese allocation to industry, and to a type of industry where the capital/labor ratio is high, might well operate the other way. Also India's emphasis upon tertiary sectors would suggest on the whole a larger increase in output from the same level of new investment, although this argument is not firm, since transport itself tends to have a high capital-output ratio, as does also investment in housing, included here under social services. On this last, one point does warrant mention. The greater allocations to services might mean more investment in the overhead sectors which in turn will permit greater returns from direct investment in the future. India's past pattern, in other words, may well be the more efficient, given a longer time

of agricultural credit cooperatives will make it possible to draw a huge amount of idle capital into agricultural production" [1, p. 33]. A delegation of Indian governmental specialists in agriculture also reported this same preponderance of agricultural investment in China relative to India [10, pp. 133-35].

horizon. However relevant this possibility, other evidence does not suggest that the rate of growth prospects for the years ahead can be considered more favorable to India as a consequence of the current investment patterns.

B. *Some Sectoral Capital Coefficients*

A few precise relationships can be traced with the data at hand. Thus, the ratio between gross real investment and the increase in real income in agriculture in India is 2.33 for the period 1951/1952 through 1956/1957. Essentially the same figure (2.28) is obtained for China for 1951 through 1957.[16] A comparison of the investment which corresponded to an increase of (productive capacity for?) one million long tons of food grains over the period also reveals a similar parallelism in the experience of the two countries. Again, there is some basis for imputing an advantage to the Chinese on the ground of relatively more consistent results over the period.[17]

While the evidence in the agricultural sectors can be interpreted as suggesting reasonably comparable capital-output relationships, for industry it indicates a clear advantage for China. The gross capital-output ratio for Indian industry and power was at least 6:1. For China, valued-added computations for industry are less readily presented, given the problem of double counting. The ratio of gross investment in industry to the *total* value of industrial production lies in the .9-1.1 range for different groups of years during 1950-57. On the assumption that total value is of the order of three times the value-added,[18] this suggests that the comparable capital-output ratio for China would be about 3:1—or half the level of India's.

The national income series in the two countries show quite different patterns with respect to the relative importance of income from agriculture. In India this ratio was 50 per cent before the plan, 47 per cent during it, but seems again to be close to 50 per cent in the recent second-plan years. The accounts for China show a fairly steady decline from 70 per cent[19] in 1950 to 50 per cent in 1957. Thus, the major

[16] The ratios appear to be more volatile in India. Thus, a much less favorable relationship is obtained if the computation for India includes the disappointing crop year 1957/58.

[17] Essentially equal results in these calculations involve an assumption that the official yuan:rupee rate of 1:2 undervalues the yuan to some extent. Other evidence on relative prices does not support this inference. See for example, [10, p. 41] [20, pp. 60-61] and above, p. 291. (For whatever relevance it has, the recent Hong Kong free-market rate of 1:1.2 suggests the opposite.)

[18] Based on computations made by W. W. Hollister (as reported in a personal communication).

[19] This figure reflects the underutilization of existing nonagricultural plant in the 1949-51 period. By 1951 the ratio was close to 60 per cent and it has been declining rather steadily since then [5, p. 9] [6].

sector where capital-output ratios seem to be more or less the same in the two countries became of appreciably less importance in China. A sector where the ratios are notably different—modern industry—has grown relatively in China. Finally, the different rates at which the tertiary sectors expand would also favor larger product, at least in the short run, in China. Together, these different sectoral results may explain the differences in over-all capital-output ratios—although nothing in the above explains these sectoral results.

C. *The Relative Scale of Public Investment*

There are important differences between India and China in the relative scales of public investment. While this direct role of government in development has been expanding at a more rapid rate in India (Table 5), the Chinese ratio for the economy as a whole in the past two years was still double that of India. Modern industry and power are today essentially entirely within China's public sector, and even in agriculture, which was relatively free enterprise in 1950, government participation is now more important than is true in India.

Does the degree of centralization of investment activity influence efficiency? Presumably, identical enterprises could be established and

TABLE 5—THE RATIO OF PUBLIC INVESTMENT TO TOTAL INVESTMENT
(Per cent)

	Preplan		First Plan				Second Plan
			First 3 Years		Last 2 Years		
	India (1950)	China (1950–52)	India (1951–53)	China (1953–55)	India (1954–55)	China (1956–57)	India (1956–57)
All Sectors	27.8	67	28.4	76.3	38.2	81	38.7
Agriculture	28.4	20.6	29.9	31.9	41.4	39.3	35.2
Industry	17.8	89.6	19.8	93.7	28.1	96.7	34

Sources:
India: My estimates based on provisional data released by government [9, pp. 154–59].
China: Estimates based primarily on [5] [25] [1].

managed with comparable efficiency by government, or by private interests or by the two combined in some mix. But limited delegation of authority in government enterprise might serve to make public operation less efficient in a relatively free-market economy. Conversely, the talents of the private entrepreneur must be less evident in an economic order in which his operations are intertwined with operations of government-controlled economic activities. On such counts alone, the output from enterprises in a mixed economy may be smaller than from the

same enterprises in a more monolithic economic order. However, the relative efficiency of the two patterns revealed in Table 5 cannot be assessed simply by evaluating the performance of public and private enterprises under different conditions. More relevant is the fact that the seemingly same type of enterprise becomes quite different in the highly centralized Chinese effort and in the much less controlled program in India.

Within agriculture, within industry, within each category of service, there is in each country a different collection of end-products which comprise output. Of the current flow of goods and services in the Indian economy the private sector still creates 90 per cent. Product must thus satisfy the demands on a relatively free market. Today, even steel is produced almost entirely in the private sector. While government allocates most of the steel available in India, significant percentages of the total supply (50 per cent or so) does not go to government. In China, essentially all steel finds its way into defense, railways and the broad public development program. Output per unit of investment must be lower for a steel industry which processes to meet the needs of many different users. This is even more true for consumer goods. Thus the modern textile industry in India produces a greater number of counts per million yards than does *any* other country's, and particularly China's. Increasingly, construction specifications for all buildings in China have been tailored to meet standards for costs per square foot established by region. India has moved much less far here.

As there are differences in product from the same type of enterprise, so also with respect to production techniques. Thus, it is relevant that China does use, in construction activity particularly, a large volume of unpaid or underpaid labor. Frequent attention has been called to the possibilities in this direction in lands like India and China where the social order generally provides for consumption even though workers are persistently underemployed. India seeks to mobilize this productive capacity through various voluntary programs, notably in the community development projects. Chinese efforts of this sort seem to have gone much farther and on a less voluntary basis [4, pp. 27-28].[20]

The predominance of public investment in China may thus contribute to lower investment costs per unit of product because government restricts the degree of choice in both factor and product markets.[21] The

[20] In India [13, p. 116] the value of all popular contributions to the community development program was of the order of 3.5 rupees per person in the project area. This would mean at the outside less than 5 days of contributed labor per person over as long as 4½ years in some villages.

[21] Many people would argue that freedom of choice and variety itself are attributes which enhance product values. The question here is only the point at which they impinge upon (physical) output in very poor countries.

private sector is indeed a decisive force in India's economy. In the plans, government has consistently overestimated the extent to which private enterprise will govern its investment and production activities "according to plan." Thus, since 1950, government has actually invested less and the rest of the economy more than was planned. A vigorous private sector has retained a greater volume of its savings for direct investment; there was a reduced transfer of private savings to the public sector. New investment in private industry has been of a more capital-intensive type than was appropriate to India's factor endowments [16, p. 29]. In contrast, China's centralized control of investment has meant investment more nearly as planned and with centralized decisions as to the techniques for production. When unplanned developments occurred—unfavorable harvests, lower grain deliveries—the control of savings in the economy gave government considerable flexibility in adapting annual programs to actual events.

D. *Contrasting Policies with Regard to Saving*

Nearly half the difference in output performance arose from China's higher investment ratios. Government has assumed responsibility for a very large part of the savings function, through an expansion in the scale and forms of taxation, especially upon agriculture, and through the growing importance of state enterprises in many fields. Thus tax receipts more than doubled between 1950 and 1952; thereafter they increased by about 50 per cent, aggregating almost 15 per cent of gross national income in each year. Limitations upon private consumption and investment meant also that private savings (especially nonagricultural) moved readily to the public sector through the banks, other saving schemes, and the bond market. Indeed, government—central, provincial, and local—in China has spent at least 25 per cent of the total national income in each year since 1952. About 11 or 12 per cent went for the usual administrative activities of government, including military services, and 13 to 18 per cent for capital maintenance and expansion [5, p. 6]. An increasing percentage of this last was accounted for by the profits and capital consumption allowances of state enterprises. The total of domestic gross savings mounted steadily (with one setback in 1955) and have averaged 22 per cent of GNP in the past few years. Government development plans envisage a slight reduction —or at most the maintenance—of this ratio in the future.

Savings in India are still essentially private. Government surpluses on current account were not expected to be significant over the second-plan period.[22] The public investment program has essentially depended

[22] The plan shows a surplus of about Rs. 10.5 billion, but this is more or less matched by current rather than investment outlays in the development program [16, p. 17].

upon borrowing domestically and abroad. Actual performance from 1956 to date has apparently intensified this situation. Revenue from taxation has varied between 7.5 and 9 per cent of gross national income since 1950. While direct taxes on agriculture (land and income) have been increasing, they still account for less than 10 per cent of the total tax revenues in 1956/1957. Rough estimates of the total tax burden for agriculture suggest an order of magnitude of about 20 per cent of all taxes, as compared with a ratio at least three times as great in China. Profits and capital-consumption allowances from state enterprises in India are still essentially confined to the traditional public enterprises—the railways and postal services. These contribute a relatively small amount to government finance. The government of India, currently responsible for less than 40 per cent of total investment in India, is thus much more heavily dependent on outside, nongovernmental sources of investment finance than is the government of China, which is responsible for more than 80 per cent of total investment in that country. The domestic savings ratios in India do not seem to have expanded since 1955, and government has had considerable difficulty in attracting them to public investment. As mentioned earlier, India's public development program over the next few years will certainly be more heavily dependent upon foreign resources than in the past, and the turning point in this dependence cannot yet be foreseen [17].

III. *Implications for Economic Development Elsewhere*

There are no magic formulae for achieving an expanding per capita income. The tasks confronting nations with very low levels of average output are most difficult at best. The rapid growth periods in India during the last few years of the first plan and in China in some of the years treated here need to be considered abnormal. They were related to favorable weather or to the exploitation of existing excess capacity. These apart, continuing progress requires persistent efforts by the people under capable and inspiring leadership.

Consider a few key problems and the different approaches to them in India and China. Most poor countries have abundant supplies of labor relative to capital. Along with the need to move toward more capital-intensive methods of production in certain sectors, there is a complementary role for labor-intensive pursuits in rural areas especially, but in cities also. The gains from expanding the productivity of labor now inefficiently used are enhanced by the fact that there do exist complementary capital resources—in existing equipment, in nonmonetized savings (such as surplus agricultural products)—which must be used locally, if at all. Despite the greater emphasis given by the Chinese to the development of modern industry, they have also devoted major

energy to the task of mobilizing these underutilized resources for plan objectives. Through a sequence of devices, culminating in today's communes, China's government has played a fundamental role in organizing local resources—labor, existing plant, raw materials, savings (especially nonmonetized) and leadership of both enterprise and public administration—to expand agricultural and industrial production.

Mention has already been made of nonmonetized savings. Wherever a significant part of national product is imputed because it never goes to market, there will be savings in the form of inventories of final product which are used directly, perhaps as payments in kind, in capital creation. In both India and China some 25 to 30 per cent of all gross investment may have occurred in nonmonetized form in 1950. This ratio has apparently not changed much in India, but it has declined markedly in China, partly because the Chinese attempt to siphon out of the rural areas whatever is or can be monetized, and partly because of the differential expansion of nonagricultural investment. In contrast to China, India does not even take explicit account of this important form of rural savings and investment, despite the fact that Indian experience has pointed up both its size and potential [23, p. 63].

Unemployment constitutes a political as well as an economic threat in underdeveloped countries. The employment objective explains part of India's preoccupation with the handicraft sector; emphasis here has even been allowed to interfere with cost and efficiency considerations in the production of consumer goods by the modern industry sector. Progress is only now beginning to be made toward the expansion of the modern small-scale sector which either competes with large industry or complements it. Great attention is now given this sector in Chinese industrial development. There are benefits to employment and output gains from using resources raised locally. The Chinese have found that emphasis on this small-scale sector of industry has also facilitated the program of regional development by providing employment opportunities away from the large industrial centers.

The inability of the large cities to provide as much employment as people seek has become most apparent, even in China with its strong stress on industrial development. The Chinese have recognized explicitly the dangers arising from the unemployment of overurbanization and have devised various measures (involving involuntary transfers of people, compulsory registration, etc.) for dealing with the problem. In this context, the emphasis on employment opportunities in the small centers and rural areas takes on greater importance. In India, the parallel dangers have not yet produced a clear policy. Growing overurbanization has tended rather to expand social overhead expenditures in

urban areas beyond what the development requirements of these centers might otherwise have been.

For some parts of the program, particularly for industry, the government of India has chosen to rely heavily upon the private sector. The experience here leaves no doubt about the major gains possible from the energy and drive of private entrepreneurs. But these can be realized over a prolonged period only when government also fulfills major investment responsibilities: in the social and economic overhead sectors, and in certain directly productive fields where private investment has limited experience and resource requirements are very high (e.g., steel and producers' goods) or where effective economic change involves a major program of social action (as in peasant agriculture, rural output generally). Fulfilling this complementary role in a society of mixed enterprise requires careful study of the flow patterns of domestic savings, and the determination to adopt policies which can appropriately influence this pattern.

Structural unemployment, underutilized resources, overurbanization, nonmonetized savings and investment flows—these are illustrative of the types of problems that must be understood and treated if there are to be steady output gains in most of today's underdeveloped areas.

Indian leadership has not yet assumed the responsibilities for organization and planning required to meet these problems. This is highlighted by the fact that the ratio of government to total expenditure is lower than in any other country for which national product statistics are available.[23] China's relative success in its development effort bids fair to be maintained, if not expanded. It is not realistic to expect internal pressures to impede progress. Indeed, apart from adverse harvests and the like, the years ahead may well see some reduction in the relative allocations of income to investment, and some increases in levels of living.

Are the contrasts in methods—and achievements—inseparable from contrasts in political philosophy? It is true that Chinese resource mobilization and allocation, Chinese methods of dealing with unemployment and urban growth, do weigh seriously upon the individual citizen and especially on a peasantry long proud of its individualism. These procedures could not be reconciled with the dictates of the Indian constitution, nor with the political and social philosophy of present-day leadership in that country. But alternative actions might well accomplish the same, or nearly the same, objectives. For reasons which cannot be attributed to India's adherence to the tenets of democracy, rural taxation

[23] For a complete listing of a related statistic, see [19, pp. 382-84]. The ratio for India is less than 1/3 that for the United States or Canada, for example.

is minimal; tax evasion is high; government controls a small proportion of the economy. The community development schemes offer an excellent and democratic mechanism for mobilizing idle or poorly allocated resources in rural areas. Economic inducements might deter the rural urban population push. The scope for such actions under democracy is broad.

The growing awareness of Chinese achievement relative to India's can have a profound influence upon world political and economic developments. But the lesson to be derived from the comparative performance of the two countries over these years of intensive development planning is not that totalitarian methods serve better than those conceived and implemented under democracy. It is rather that government in nations aspiring to economic expansion needs to define the tasks of growth realistically; more, government must implement them faithfully.

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POPULATION AND ECONOMIC GROWTH

By EVERETT E. HAGEN*

It is likely that the disagreement that occasionally arises concerning population growth theory and "the population problem" is due in part to failure of communication, resulting from unclear statement of assumptions; but also in part to the persistence in our minds of Malthusian conclusions, even though the assumptions on which they are based may have been forgotten. I hope that this paper will eliminate at least some of the disagreement.[1]

In some analysis all population increase is lumped together in one general case: if per capita income rises above subsistence, population grows. It is conducive to clarity of thinking to treat causation more precisely, and to distinguish three cases of population growth.

Two of these relate to low-income (peasant) societies. In such so-
1. cieties, crude birth rates are virtually everywhere above 40 per thousand. Although higher birth rates are biologically possible, rates of 45 per thousand are close to the practical maximum. Historically, death rates have been almost as high. Where population growth occurs in such societies, it occurs because death rates fall. One cause of falling death rates is the introduction of modern public health and other preventive medical measures. I shall term the resulting fall in death rates exogenous. The fall in death rates is followed only after a lag of undetermined but considerable length by a fall in birth rates. In the interval the rate of population growth rises.

2. A second cause, marking the second case treated here, is a rise in per capita income.[2] Historically, there have been two major causes of rise in *aggregate* income, the opening up of new territories and technological progress. The former merely enlarges the scale of the economy and permits it to support more persons without directly affecting per capita income.[3] Technological progress, however, in general directly

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[1] I do not deal with questions of optimum population size or factor proportions in a static setting, or with questions of cyclical change in population-growth rates.

[2] Of course the availability of public health and other preventive measures constitutes a rise in income. The reference here is to a rise in general purchasing power.

[3] The filling of "empty lands" was conspicuous in Western Europe during the Middle Ages, in China after 1650, and more recently in the history of the United States, Canada,

increases per capita income. People live more healthfully, the death rate falls, and the rate of population growth increases. If it increases until it equals the rate of growth in aggregate income, per capita income of course ceases to rise. With birth rates between 40 and 45 per thousand, a decline in death rates to Western levels could bring population growth at the rate of 35 per thousand, or 3.5 per cent per year.[4]

The recent spurt in population growth in the West constitutes the third case. By the 1920's or 1930's, the "demographic revolution" in all Western countries had been completed, and birth rates had reached secular minima. Early in the postwar period, the rates in most Western European countries and in the United States, Canada, Australia, and New Zealand rose markedly above those minima. For a time this phenomenon was regarded as the making good of deferred births, but it has now continued so long, and family sizes have so clearly increased, that at least part of the rise in birth rates must be regarded as reflecting some secular change.

If an analytical model is to be of general applicability, it must be consistent with all three cases. Section I presents historical data indicating that, contrary to Malthusian expectations and common belief, income-induced population growth has nowhere prevented even a moderate rate of rise in the aggregate income of an economy from bringing continuing rise in per capita income.[5] A reasonable interpretation of history suggests a mechanism at work which has guaranteed that it would not do so. Section II presents a model incorporating this mechanism. Section III comments briefly on the Malthusian model, and Section IV relates my alternative model to the three empirical cases and discusses its relevance to the future.

I. *Historical Population Growth: the Data*

The historical distinction between exogenous and induced population growth is fairly clear. Exogenous declines in death rates have attracted attention mainly since the second world war. Through technical assis-

Australia, and New Zealand. In all of the recent cases except China, continuing technological progress was of course occurring at the same time. Even where there is no progress in known technology, there may of course be economies of scale which increase productivity. I shall not discuss their relation to population. The reader will be able to apply the argument of Section II.

[4] Life expectancy in Western societies is consistent with a death rate of 14 or 15 per thousand even with a stabilized age distribution of the population; and crude death rates of 4 or 5 per thousand are possible with population concentrated in the lower age groups, as it is for some time after death rates decline. Birth rates of 50 or 55 per thousand are biologically possible, and have prevailed in limited areas for periods of moderate length. Thus a rate of population growth much higher than 3.5 per cent per year is conceivable. However, this rate may be taken as a practical maximum.

[5] Of course income-induced population growth may prevent the rise in per capita income from being as fast as it otherwise would be.

tance from abroad, death rates in a considerable number of peasant countries have fallen drastically, most abruptly of all in Ceylon. Birth rates have remained high, and as a result in recent years population growth has been approximately 3 per cent per year, or more, in Ceylon, Malaya, Mexico, Venezuela, Ecuador, and several Central American countries and Caribbean areas, and well above 2 per cent in a number of other countries. Because of the recency of the declines in death rates, the minimum length of the lag before birth rates fall is unknown, except that it is probably more than a decade.[6] In the historical cases of less spectacular fall in death rates, the lag has been as long as several generations. If it should now be a generation or longer, the effect on living levels in low-income countries where rapid technological progress is not occurring may be catastrophic.

It is not so widely observed that the slower but increased rate of population growth in a number of peasant countries over a longer period has been due to the same mechanism. From the beginning of the Christian era to 1650, the average rate of growth of world population was in the neighborhood of 1/20 of 1 per cent per year. It then began to rise, first in Western Europe, but during the last half of the nineteenth century also in the peasant societies, which were then colonial. The modal rate in peasant societies between 1900 and the second world war was probably between .5 and 1 per cent per year. While precise data are of course not available, historical evidence indicates rather clearly that the level of per capita income in such societies had not risen before the rise in the population growth rate. There is also historical evidence that the increased rate of population growth has resulted specifically from gradual introduction of improved medical and health practices under colonial administration.[7]

Because some observers point to this population rise in peasant societies as evidence that a rise in per capita income may be swamped by population growth, it is worth while to assert specifically that the rise in aggregate income in peasant societies within the century before the second world war was predominantly a result, not a cause, of popula-

[6] The crude death rate in Ceylon, as recorded in the United Nations *Demographic Yearbook,* fell from 19.8 in 1946 to 14.0 in 1947, and continued down to below 10 in 1956. The crude birth rate, which remained between 38.4 and 39.8 from 1946 to 1953, fell from 38.7 in 1953 to 35.7 in 1954, giving possible hope of a spectacularly short lag. But in 1955 it was 37.3 and in 1956, 36.4. Age-specific birth and death rates for these recent years are not available.

[7] In China, by way of exception, population growth presumably resulted primarily from introduction from abroad of the sweet potato, peanuts, and early-ripening rice, which permitted the settling of large land areas that would not previously support population.

tion growth. The forces at work in these areas tending to increase the income of the mass of the population were extremely weak. Colonial administrations did not induce continuing technological progress in the areas they controlled.

Continuing technological progress begins when an adequate base of scientific-technical knowledge is available (a condition now everywhere fulfilled), and when social and psychological changes have occurred such that a sufficient number of the individuals of the society devote their energies to problems of technological innovation. Certain conditions of capital supply are also necessary; they may be independent conditions or may follow if the social and psychological conditions exist.[8] During the nineteenth and twentieth centuries, these conditions came to exist in twenty-some countries of the world. Continuing technological progress began, and brought an accelerated rate of increase in aggregate output. As this increase got under way, per capita income gradually rose. The death rate gradually declined. The rise in income permitted maintenance of larger and larger families, if desired. If the simple Malthusian model of population behavior[9] were realistic, as per capita income rose in each of these countries, at some point population growth would have reached a rate equal to the rate of growth in aggregate output, thus checking the rise in per capita income. Or, alternatively, if the rate of rise in aggregate output was so fast that it exceeded the realistic maximum biological rate of population growth—say 3.5 per cent per year—population growth would have reached that rate. The historical facts of income and population growth will test the thesis.

I shall examine the facts only for countries in which a moderate or rapid rate of rise in aggregate output (1.5 per cent per year or more)[10] began before the end of the nineteenth century, since where it began only in the twentieth century it is possible that population growth has not yet reached its peak. There are only 17 such countries. Simon Kuznets [6] in a recent compilation presents data concerning growth of output and population for 13 of these (see Table 1). While data for the growth of output in the nineteenth century are not available for the

[8] Some economists believe that technological progress begins when certain economic barriers, bottlenecks, or vicious circles are broken, and they ignore the social and psychological factors. This difference of opinion concerning the causal factors is not important for the present purpose, but it is important to note that technological progress, not merely capital formation, is in point. The hypothesis that socio-psychological changes are of central importance does not rule out the necessity of political change. This may in some cases be necessary in order that the new groups can be free to act.

[9] I use the term "Malthusian model" loosely here. For a statement of the essential elements of the Malthusian model, see Section III.

[10] I include Ireland even though its rate of increase in output was slightly lower.

TABLE 1—POPULATION AND NATIONAL PRODUCT, 13 DEVELOPING COUNTRIES: PERCENTAGE CHANGES PER DECADE, AND DECADES OF HIGHEST PERCENTAGE POPULATION CHANGE

Country (1)	Period[a] (2)	Percentage Change per Decade		Decade of Highest dP/dt	
		National Product (3)	Population (4)	Approximate Dates[a] (5)	Percentage Change (6)
Australia	1886/94–1945/54	26[b]	17[b]	1890–1900	21.5
Canada	1870/79–1950/54	41.3	18.3		
	1870/79–1905/14	47.1	17.8	1899–1909	30.2
Denmark	1870/78–1950/54	30.1	11.5		
	1870/78–1904/13	32.7	11.3	1913–1923	17.5
France	1841/50–1949/53	15.3	1.3		
	1841/50–1901/10	18.6	1.9	1855–1865	4.7
Germany	1860/69–1950/54	27.4	10.1		
	1860/69–1905/14	35.6	11.5	1894–1904	15.2
Ireland-Eire	1860/69–1949/53	12.8	−3.5		
	1860/69–1904/13	11.6	−5.4	1938–1948	0.0
Italy	1862/68–1950/54	18.0	6.9		
	1862/68–1904/13	15.7	7.0	1923–1933	8.0
Japan	1878/87–1950/54	42.3	12.7		
	1878/87–1903/12	49.2	11.6	1937–1947	14.7
Sweden	1861/68–1950/54	36.0	6.6		
	1861/68–1904/13			1938–1948	12.5
Switzerland	1890/99–1939/48	21[b]	7[b]	1894–1913	10.5[c]
Russia-U.S.S.R.	1870–1954	31.0	13.4		
	1870–1913	27.7	15.7	1870–1885	15.3[d]
United Kingdom	1860/69–1949/53	21.5	8.0		
	1860/69–1905/14	25.0	11.1	1869–1879	12.4
United States	1869/78–1950/54	41.2	17.4		
	1869/78–1904/13	56.0	22.3	1873–1883	24.7

[a] Generally, Kuznets presents data for overlapping decades. His population data are shown as for intervals from one overlapping decade to another. The dates given in col. 5 are the fifth years of the decades he cites.

[b] The decade rate from the first to the last period covered. Other percentages in these columns are trend-line rates.

[c] The decade rate for the 20-year period.

[d] The decade rate for the 16-year period.

Source: Cols. 5 and 6, Kuznets [6, Appendix Tables 1-5, 7, 9, 10, 13-15, 17-18]; cols. 2-4, [6, Table 2, except Australia and Switzerland, which were computed from Appendix Tables 18 and 5, respectively].

other 4 countries,[11] it is clear from general historical comments about those countries that the course of events in them paralleled that in the other 13. In each country, the death rate fell as nutrition, health care, etc. improved. The rate of population growth would have approached the biological maximum if birth rates had simply remained at their previous level as income rose. What happened?

1. In no country except the United States and Canada—where vast empty lands cried out to be filled—did rise in income stimulate population growth remotely approaching that which Malthusian theory indicates rising income should induce. In no case except those of "empty lands" did the rate of population growth exceed 17.5 per cent per decade even for a single decade.[12] The median peak decade rate among the 13 countries is 12.5 per cent. The median rate for the 50 years of fastest growth is much lower.

2. In no country did the rate of population growth approach the rate of growth in aggregate output. In fact, in none except the United States did the *peak* rate of population growth for a single decade approach the *average* rate of growth in output for the entire 50- to 100-year period covered.

3. In England, there is evidence that in the eighteenth century, before the period of most rapid growth, rise in aggregate income began slowly and accelerated only gradually. Various studies give conflicting evidence about the timing of the rise in output, but all indicate that over the century as a whole, output rose. Phyllis Deane's estimates [2] [3] indicate an average rate of growth for the century of about 15 per cent per decade, but she thinks the growth was concentrated in the first half of the century. Population growth during the century failed to keep pace even with this moderate rate; it rose by about 6 per cent per decade.[13]

The evidence thus indicates not merely that the "Malthusian" result did not occur generally, but that it did not occur anywhere. Instead, birth rates followed death rates downward long before a maximum rate of population increase had been reached, and both continued downward until they reached secular minima.[14]

[11] Belgium, the Netherlands, Norway, and New Zealand. For all but Belgium Kuznets presents data for the twentieth century. In the twentieth century, rapid growth began in a number of other countries, for example, Czechoslovakia, Poland, Mexico, Brazil, Colombia, and perhaps also Hungary, Turkey, Argentina, and Chile. Some other Latin American countries, and possibly one or two elsewhere, might be added.

[12] In Australia, New Zealand, the United States, and Canada, during an early preindustrial period of filling empty lands, population growth reached higher rates than those shown in Table 1, but this growth is not relevant to the present argument.

[13] Concerning other estimates of income or production, see T. S. Ashton [1] and sources cited by him.

[14] See the sources cited by Kuznets [6]; or for a general description of the trend see any standard demographic discussion, such as R. R. Kuczynski [5].

These facts raise the important question whether technological progress is apt to bring such favorable results wherever it begins in the future. The uniformity of the past result suggests that there was some causal mechanism at work. It is worth while to inquire what that mechanism may have been.

II. *An Analytical Model*

Let aggregate income be indicated by *Y*, population by *P;* let *v* and *r* respectively represent the percentage change per time period in aggregate income and population. In Figure 1 income per capita ($\dot{Y}/P$, or *y*)

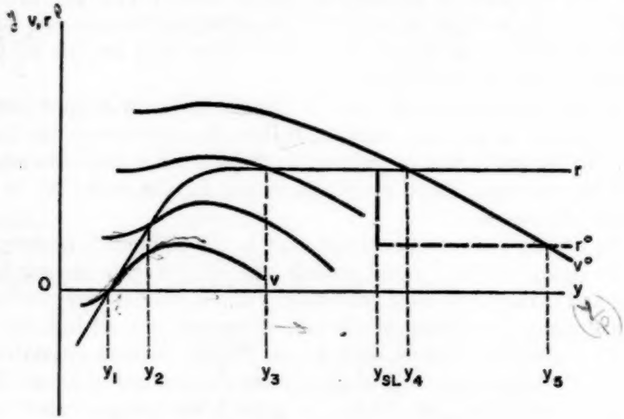

FIGURE 1. THE SIMPLE "MALTHUSIAN" MODEL

is shown on the horizontal axis, and rates *v* and *r* (abstract numbers) on the vertical.[15]

Full employment is assumed, and for simplicity the level of employment is assumed to bear a constant ratio to population. The *r* curve is assumed to rise with rising *y* until it reaches a maximum set by biological limitations (the practical maximum rather than the highest conceivable level), after which it remains horizontal.

Output is a function of capital and labor inputs only. "Land" will be introduced later. A conventional production function and constant

[15] The axes are those of Figure 3a in R. R. Nelson's article [9, p. 900], which seems to me the sharpest delineation in print of the Malthusian model. The *r* curve is his curve, slightly adapted. For two other articles that use the graphic device of rates on one axis to deal elegantly with problems of economic growth, see R. M. Solow [10] and Trevor Swan [11]. I use a *v* curve here instead of the more elegant Solow-Swan sY/C curve (s = ratio of saving to income), because the latter becomes unmanageable when land is introduced into the argument.

returns to scale[16] are assumed. Each v function is a long-run function reflecting a constant state of the arts and a constant function relating saving (S) to income. An upward shift of the function results from technological progress or an upward shift in the S/Y function or both. Movement along a v function from a lower to a higher y results solely from an increase in the ratio of capital inputs (C).to labor inputs (P). The shape of the v curve depends on the operation of the law of variable proportions as the C/P ratio varies with the advance to higher levels of y; for at different levels of y the rate of saving and the rate of population growth will differ, thus producing corresponding changes in the C/P ratio. The specific shape assumed does not affect the argument here, so long as the curve intersects the r curve from above, as in Figure 1.[17] It must intersect thus to reflect the Malthusian assumption of a tendency for population increase to check the rise in per capita income. If by a fortunate accident or a lump contribution of capital from outside the system, y were raised above y_1, for example, increase in population at a higher rate than in aggregate income would force it down again. This is the "Malthusian trap."[18]

It is assumed that there is some value of y at which both saving and population growth are zero. This assumption, which is not in the least necessary, satisfies the simplest form of the Malthusian notion of a subsistence level of income. In Figure 1, let this point be y_1. Suppose now that through some exogenous force the v function shifts upward. In this two-factor model, v and r will reach a new stable equilibrium, as at y_2. Population, capital, and aggregate income will increase indefinitely at a constant proportional rate, while y remains constant at an increased level. If the intersection is in the horizontal section of the r curve, as at y_3, then capital, population and aggregate income race upward at the same rate, and one also equal to the biological maximum possible rate of population increase. The rapid increase in population (labor inputs) causes no tendency for y to decline, since labor force increase runs into no scarce cooperant factor.

[16] A different assumption concerning returns to scale slightly complicates the statement of the argument without affecting the conclusions.

[17] The more steeply the curve rises to the right of its intersection with the r curve, the less the upward shift necessary for it to arch above the r curve, eliminating the low-income-level intersection, and thus escaping the Malthusian low-income-level-equilibrium trap. Similarly, if the v curve arches upward sufficiently, at the right of its intersection with the r curve, it may intersect the horizontal portion of the r curve from below, creating an unstable equilibrium that will permit escape from the low-level-equilibrium trap if some force pushes the system up beyond the point of unstable equilibrium. (See R. R. Nelson [9]). Since the empirical evidence indicates that the low-level equilibrium has not been escaped in either of these ways anywhere in the world, we are interested here only in the area in the vicinity of the low-level equilibrium; hence the precise shape of the curve is not important.

[18] The phrase "Malthusian trap" is Nelson's [9].

Suppose, however, that we assume the existence of a third factor, land, which is augmentable only at increasing cost per unit (or, if you choose, is available in absolutely limited supply). Then the value of v at any given level of y will depend not only on the level of techniques and the C/P ratio, but also on the quantity of C and P. As C and P increase, the marginal productivity of increments of capital and labor (for convenience think of a combined unit of capital and labor) will diminish after a point, and after a further point the value of v for any given level of y will fall. This fall in v without change in the ratio of C to P constitutes a downward shift in the v function.[19] This downward shift, inevitable in the absence of a renewed force tending to push the function upward, will continue until y has returned to the subsistence level.[20]

Above some level of y, birth rates may fall because of a "standard of living" effect. That is, the attainment of this level of per capita income in some way induces a fall in birth rates.[21] In Figure 1, let y_{SL} be this point. Then if the v curve shifts upward to the position v^0, intersecting the r curve to the right of y_{SL}, as at y_4, the effect of experiencing this level of living will be to shift the r curve downward to the position r^0, and continued capital formation even without technical advance will cause y to rise to y_5. However, y will thereafter decline if the limitation on land supply continues to exert its influence, since that limitation will press the v curve steadily downward and thus cause y to move steadily to the left.

In the model described thus far in this section, which reflects fairly conventional assumptions about population growth, as per capita income rises the rate of population growth rises to the biological maximum. Further, per capita income turns downward secularly. Historically, neither phenomenon has occurred. A mechanism that will make the model more relevant to reality is created if we assume that technological progress continues throughout the time period being con-

[19] And is to be contrasted with movement along the curve because of change in the C/P ratio.

[20] The simple assumption of land augmentability only at increasing cost is not realistic. The model is more relevant to reality if instead it is assumed that technological progress increases the quantity of land, making the upward or downward drift of the v function a matter of the specific parameters employed. However, it may be well to carry through the analysis on the assumption of land augmentable only at increasing cost, since this is Malthus' assumption.

[21] Only a very broad assumption about the causal mechanism involved is necessary at this point. It should not be assumed that the direct operating force affecting birth rates is necessarily the increase in per capita income. The rise in per capita income is accompanied by a shift in the occupational composition of the labor force, urbanization, and a decline in death rates. Somewhere within this complex is the force that produces the standard-of-living effect. A specific assumption about the nature of the causal mechanism is made immediately below.

sidered and also posit the hypothesis stated below concerning the determinants of birth rates.[22] The hypothesis has two elements, a modified standard-of-living effect and desire to perpetuate the family.

1. The level of birth rates is determined by that of death rates. Birth rates are adjusted to death rates by the intention of the typical family to have two children grow up to parenthood so that the family will be perpetuated. Actually, to allow for the uncertainties of human life, and because in most cultures it is desired that one of the two children who perpetuate the family be male, the typical aim is to raise somewhat more than two to parenthood. Where death rates are at a level that birth rates cannot exceed, this intention is of course frustrated. But where conditions of sustenance permit, the basic long-run rate of population growth is a fairly low positive rate. In equilibrium it does not tend to rise above that, because it is limited by standard-of-living considerations. ("Fairly low" may be a rate varying among different societies between .5 and 1.3 per cent per year.)[23] It would not be inconsistent with this formulation to find this rate positively correlated with per capita income, but I find no empirical evidence that it is.

2. This birth rate and size of family calculus is, however, not primarily conscious and rational (though it is rationalized), but is imbedded in unconscious motives[24] relating to sex and family inculcated in children during their first six years.[25] Hence the minimum lag between

[22] The relevance of the model to reality is increased by a simple extension of the conventional standard-of-living thesis, namely, the assumption that a standard-of-living effect occurs at any level of per capita income above subsistence, if that income level is experienced for a minimum period. There is no theoretical justification for assuming any specific floor of per capita income below which a standard-of-living effect does not occur. This assumption eliminates the rise of the population growth rate to the biological maximum, except in the case of a very rapid fall in death rates. We should not, however, rest with this extension of the standard-of-living thesis, since further specification of the determinants of birth rates creates interesting results in the model.

[23] For consistency with the findings of modern social science, we must assume that this minimum rate is culturally, not biogenically, determined, and varies among societies. Ireland and China may perhaps be taken respectively as extreme cases of a low and a high tendency to population increase.

[24] I use the word "motive" in its technical psychological sense, as equivalent to the other technical terms "need," "motivation," or (as used by some psychologists) "drive." It does not refer to a conscious purpose.

[25] Perhaps during the "genital" period of the ages 3 to 6 years, when the relationship that Freud termed the "family romance" appears. Concerning this period, see, e.g., O. S. English and G. H. J. Pearson [4, Ch. 5]. The speculation that this period may be important in the determination of birth rates, while consistent with present-day theories of personality formation, is purely my own. The assumption stated in the text is oversimplified. Certainly the relevant motives are reinforced, or in the exceptional case altered, with resulting inner conflict, during adolescence. Thus during traumatic periods, birth rate changes might occur within a decade of events causing them, such as war. Such traumatic causes of change are ignored here.

a fall in death rates and the resulting fall in birth rates is the period from age 6 to parenthood. Further, since these sex and progeny mores are transmitted unconsciously, ones appropriate in association with high death rates may be transmitted for up to say four generations after death rates begin to fall. Between these limits, the upper one of which is based solely on historical data, the length of the lag varies inversely with the speed and conspicuousness of the decline in death rates. In England, where the decline was slow, gradual, and for some time hardly noticed, the lag was four generations; in Western Europe, it was shorter; in low-income societies now experiencing rapid and spectacular declines, it may be the minimum.

The alternative assumptions that a decline in birth rates depends on a rise in per capita income and on a fall in death rates are both consistent with the historical evidence, for until recent generations a rise in per capita income and a fall in death rates have occurred together. The latter assumption seems to the writer more consistent with modern psychological theory, but an empirical test must await another generation.

Since any single technological improvement will tend to shift the v curve upward, continuing technological progress at any rate whatever will counteract diminishing returns sufficiently to hold per capita income above subsistence and cause a standard-of-living effect, though if land stringency is great and the rate of technological progress very slow, the system may return virtually to the subsistence level of y within a few years, and the decline in the birth rate may be negligible. With a faster rate of technological progress, which holds the v function appreciably above subsistence for a sufficient time, there will follow a noticeable decline of death and birth rates until both reach minima, after which a rise in per capita income may continue.

The process is illustrated in Figure 2. Let us assume for graphical simplicity that the v function rises to its position 2 and hovers at that level. The upward shift causes a rise in y and r to the intersection b of the r_1 and v_2 functions.[26] The rise in r was of course via a fall in death rates. At the higher level of y, after a lag, birth rates also will fall. The resulting fall in the rate of population increase constitutes a fall in the r function, as to position r_2. The value of r falls from b to c. Since r is now less than v, y and r again rise, this time to its intersection d of the r_2 and v_2 curves. The rise in r is via a further fall in death rates. A further fall in birth rates then occurs, and the process is repeated, r moving from d to e to f to g to h.[27] Here the process stops,

[26] The value of y and r will reach this short-run equilibrium via a path which lies between the two curves.

[27] While Figure 2 portrays the process as a "ratchet" effect, the downward shift of the r function and the rightward shift of the values may be expected to be continuous.

if we assume that in position 4 of the r function death rates (and therefore birth rates) have reached a minimum. Before death and birth rates reached their minima, the system passed through a period of considerable population growth, but the characteristics of the model guaranteed that as aggregate output rose population growth would not rise as fast, i.e., that per capita income would rise cumulatively.

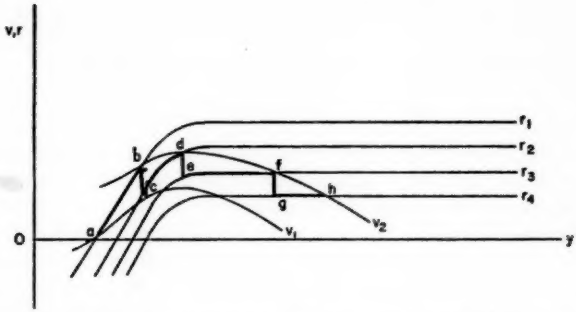

FIGURE 2. THE STANDARD-OF-LIVING EFFECT AT A LOW-INCOME LEVEL.

Before the system reaches point h, technological progress may have created new land (new minerals, cheaper ways of extracting them from low-grade deposits, plastics, new energy sources), thus pushing all or part of the v function upward; or may have created new substitutes for labor (labor-saving inventions), thus flattening out the concavity downward of the function (raising the right-hand portion of the curve), and pushing the intersection h to the right. For one or both reasons, the point h may recede rightward, and rise in per capita income may continue. This has happened to date in the technologically progressive countries of the world, and the two tendencies show no evidence of coming to an end or even slowing down.

We may next consider the effects of exogenous declines in death rates in a system at the subsistence level of income. When improved medical and health measures are introduced, it becomes possible for the population barely to reproduce itself at a lower level of per capita income. Graphically, in Figure 1 the r curve, retaining its former general shape, shifts to the left. There is a new subsistence level of income, the intersection of the v and r curves with the latter in its new position, somewhere to the left of y_1. There is now population growth at the income level y_1; it will cause per capita income to fall progressively until the new subsistence level is reached. In the process, death rates will rise again, starvation and diseases associated with malnutrition replacing malaria, venereal disease, etc., as major causes of death. Birth rates

may or may not fall during the shift, depending on the time period involved before the new equilibrium is reached, but in any case they will remain above death rates and the new equilibrium will inexorably be approached.[28]

If such an exogenous decline in death rates is brought about in a system experiencing technological progress, the increased rate of growth in population will tend to reduce per capita income, but equilibrium will be reached above the new subsistence level, and the process sketched above of reduction in death and birth rates to their minima and then continuing growth in per capita income will occur.

III. *The Malthusian Model*

The reader, if he has a Malthusian model in mind, will have observed that the model presented here differs from it in various particulars. This is obvious even though Malthus' presentation in successive issues of the *Essay* [7] and in the *Encyclopedia Britannica* [8] varies sufficiently in detail and is sufficiently lacking in rigor so that it is difficult to reach an agreed-on simple statement of the model. But perhaps the following five points, either because of explicit statement by Malthus or by the requirements of the model, may be regarded as essential.[29]

1. The birth rate is not influenced by the level of income, and implicitly not by death rates. As a result, a decline in the death rate simply increases the rate of population increase.
2. Land is augmentable only at increasing cost.
3. A law of variable proportions operates.
4. Some sort of restriction on capital accumulation operates, so that capital accumulation cannot offset the limitation on quantity of land sufficiently to prevent per capita income from falling as population rises.
5. Technology is constant—or, technological progress is not related in any causal way to population growth, but if it occurs is only a coincidental development that may for a time stave off the results of diminishing returns.

The initial statement of a model associated with Figure 1 incorporates these assumptions, with the exception that a neo-Malthusian standard-of-living effect at a fairly high level of income is posited. It follows, as it does from any rigorous formulation of a Malthusian model, that if some force (not explained by the model) causes growth

[28] It is possible to assume that population growth, by "stirring up the society," induces a rise in the v function and prevents this result. The assumption however seems unrealistic and uninteresting.

[29] I am indebted to H. J. Barnett for suggesting, in conversation, this summary of the Malthusian system.

in aggregate income, it can increase per capita income much above subsistence only if the rate of growth in aggregate income exceeds the maximum possible rate of population growth, or if moral self-restraint concerning procreation reduces the birth rate. The unrealistic assumptions would not be objectionable if they resulted in an analysis that satisfactorily explained reality. But the contradiction between Malthusian analysis and the facts of population history suggests that the Malthusian model is an impediment, not an aid, to our thinking about population.

Important Malthusian strains have persisted in our thinking and in university teaching. I suspect that they have persisted largely because the model predicts population growth, population growth has occurred, and we have not had a satisfactory alternative explanation. It is of course important to retain the simple notion that rise of income above subsistence tends to result in population growth. But we need to analyze that growth by tools more relevant than the Malthusian ones.

IV. *Applications of the Model*

In conclusion, consider the application of the model presented here to the three empirical cases of population growth. An important application of the model is to the case of continuing technological progress. Previous doctrine has suggested that a rise in per capita income may itself induce population growth that will check the rise in the income level. The model presented here suggests that wherever continuing technological progress occurs, it will tend continuously to raise per capita income, and without an interval of population growth at the biological maximum rate. This implication of the model seems a reliable indicator of the course of events in the present low-income countries, if population growth is induced only by rising per capita income. The model suggests further that if technological progress is sufficient so that growth in aggregate income keeps pace with exogenous growth in population, after a period birth rates will follow death rates downward. Per capita income will thereafter steadily rise.

However, in many low-income countries, population growth is now occurring because of improved health and medical measures, and technological progress is not sufficient to prevent a fall in per capita incomes. The model suggests slightly greater hope for avoiding population catastrophe in such societies than does the conventional standard-of-living thesis, for the model suggests that the decline in the death rate may lead after a lag to a decline in birth rates, even though per capita income is falling. Even so, if the minimum lag between fall in death rates and that in birth rates is almost a generation, the increase

in population in the meantime may create serious economic, social, and political problems. Further, even after birth rates fall they may be expected to remain somewhat above death rates at any level of income above the new subsistence level. In the absence of technological progress or deliberate measures to reduce birth rates, per capita income may be expected to continue to decline. The implications of the model are more optimistic than those of the conventional model only in that it implies in some cases a longer period of grace, before per capita income falls to the subsistence level, in which to attain technological progress.[30]

Before the model can be used to forecast the trend of population in countries where birth rates have risen markedly after the second world war, it is necessary to test the consistency of the model with that development. F. W. Notestein has observed[31] that with respect to Western Europe and the English-speaking countries, the post-world war phenomenon is a dual one. With the conspicuous exception of five countries, Norway, the United States, Canada, Australia, and New Zealand, a new equilibrium in birth rates is being approached. Gross reproduction rates in the other countries involved are now tending toward a level roughly 15 per cent above the secular minimum. All but the five countries lie close to a regression line relating the ratio between average gross reproduction rates of recent postwar years and those of 1935-39 to change in the gross reproduction rate since 1950. Where the ratio is above 1.15, gross reproduction rates are falling; where below, they are rising. The five countries, however, lie far above this regression line.

[30] In Western Europe before the "vital revolution" associated with the industrial revolution occurred, birth rates were around 35 per thousand, or almost 10 per thousand below present birth rates in present peasant societies. Some observers see in this fact a cultural (or, conceivably, biogenic) difference that bodes ill for the peasant societies. The higher birth rates of present peasant societies result in a higher rate of population increase for any given level of death rates. This alone is extremely important. It is also possible that the higher birth rates may be associated with a greater resistance of birth rates to the downward pressure of decreasing death rates. On the other hand, it is likely that birth rates in Europe during the Middle Ages, for which we have qualitative historical knowledge but no data, were also above 40; and that the successive technological improvements of the Middle Ages or the sharp rise in living levels that followed the Black Death, had reduced death rates for a sufficient time to cause a fall in birth rates to about 35, long before the continuing technological improvement of the industrial revolution initiated further decline. If so, there is no reason to assume any fundamental difference in behavior between present peasant societies and Western Europe, though of course variation among individual societies is to be expected.

In some societies the mechanism by which birth rates were held to around 35 was late marriage. This is not inconsistent with the thesis just stated. Delay in marriage, which preserved an increased level of living, may have been a reaction to a reduced need for procreation.

[31] In conversation with the writer.

Even before I learned of Notestein's observation, I had assumed that because of the lag in adjustment of the relevant motivations to objective circumstances, the decline in birth rates might overshoot the equilibrium level and then reverse itself a generation later.[32] Acceptance of this "overshooting" thesis, however, leaves the cases of the five countries to be explained.

Four of these exceptional cases are among the six countries with the world's highest per capita incomes.[33] This suggests that there may be a positive correlation between per capita income and the rate of population growth which is concealed by other influences until the demographic revolution is completed.[34] However, this thesis is contradicted by intercountry comparisons for the 1920's, 1930's, and 1950's, and it seems necessary to reject it.

Alternatively, it is possible that among the satisfactions to which an increase in income from one generation to the next will be devoted is that of having a larger number of progeny. A rise in income will tend to cause a rise in the population growth rate via a declining death rate, if the death rate is in fact declining, and via a rise in the birth rate, after decline in the death rate has ceased. Under this hypothesis the population growth rate will tend to fall back to the basic level referred to above (p. 319) if the rise in income comes to an end, even though the higher level of income persists. On grounds of personality theory rather than of population data, this thesis is plausible. Casual observation suggests that it is in general agreement with historical facts. I have not attempted a more rigorous test of it.

Another possible explanation is that the recent rise in birth rates results from some immediate connection between creative energy and sexual activity, and that the sources—whatever they may be—of the creative energy which has lifted output rapidly after the second world war are also the sources of the current desire among young parents for larger families. Some psychologists studying personality believe that there is a close connection between creativity and sexual activity.[35] So far as our uncertain historical knowledge indicates, this hypothesis is consistent with the behavior of population during early periods of industrialization. Acceptance of this thesis, however, requires an explanation of why birth rates in other countries of Western Europe now

[32] This is a refinement not introduced into the model above.

[33] The other two being Great Britain and Switzerland.

[34] In research begun while each was a graduate student at the University of Chicago, G. S. Becker and R. E. Weintraub believe that they have found evidence to support this thesis. Each has presented his findings in a manuscript not yet published.

[35] According to F. W. Barron, in conversation with the writer.

economically vigorous are not higher, and the thesis can be fitted to the facts of the 1920's only with some difficulty.

Therefore, while in the writer's judgment the hypothesis concerning the effect of rise in income level has support in personality theory, it may be necessary to state simply that the rise in birth rates after the second world war constitutes a shift in the population function resulting from the successive stresses of depression, war, and postwar tensions.[36] The hypotheses that suggest themselves now concerning that shift are not inconsistent with those presented above relating to other aspects of population growth. Since this is true, it is reasonable to apply the model presented here to the high-income industrialized countries of the world as well as elsewhere.

A word about the very long run is in order. Even at a rate of growth of 1 per cent per year, the world's population would double in 70 years, quadruple in 140, reach a mass equal to that of the earth in a few thousand years, and so on. But this fact does not seem to me the cause for alarm that it appears to some observers. These observers do not seem to have taken account of the prospective trend in technology. In the countries of the world where population has grown the fastest during the past 150 years, the advance in technology has far outstripped it, and per capita income has steadily risen. There is reason to assume that this relationship between technological progress and population growth will hold true in the future for the entire world. Technological progress is now spreading to the other major countries of the world, and during the twentieth century will probably have spread to virtually all countries. There is no reason to place any given limits on the creation of resources by technology. One of the determinants of the rate of technological progress is the base of existing scientific and technical knowledge; increasingly rapid expansion of that base provides a foundation for increasingly rapid advance of technology, and without any definite limit. While population is reaching a mass equal to that of the earth, technology may progress so that subsistence and energy are obtained from the entire known universe, or several such universes, and only one man in a thousand may be living on the earth.

Of course, in the very long run human beings may be devoting less of their energies to technological progress, and it may slow down. But population functions in the far future may also be radically different from those now operating. One may well feel deep concern about the intermediate-run problem in certain low-income countries that are not experiencing technological progress, but it seems reasonable to be more concerned with any of a dozen other world problems than with that of prospective "standing room only."

[36] See n. 25, above.

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THE SHAPE OF THE INCOME DISTRIBUTION

By Stanley Lebergott*

A good deal of received theory on the distribution of income rests flatly, if a bit uneasily, on the truism that "the" distribution of income is highly skewed. Since Quetelet and Galton emphasized how many human characteristics were normally distributed, the contrasting skewness of the income distribution has presented something of a problem. Yet, as Tinbergen put it, "no generally accepted interpretation of the statistical regularities seems to exist. . . . This is the more remarkable since the inequality in the income distribution is at the bottom of some of the most important problems of economic policy" [38, p. 156].

We have not, of course, lacked for explanations. Some stipulate component distributions which, joined together by one analyst, may not be torn asunder by any other. Some invoke a mysterious necessity that inheres in the Pareto coefficient. Many rely on the theory of noncompeting groups. Few explanations are without merit; few without some relevance to the way in which income is distributed in the United States today. But a closer examination suggests that most explanations fail to explain. Some are too effective. They describe with equal facility, and by identical functions and parameters, the distribution among Prussian taxpayers in 1852 and American families in 1958. Others explain only part of the distribution—the part most readily fitting a particular mathematical function. Some premise social or economic structures that differ massively from those that surround us. Others report an economy seen only by the light that never shone on land or sea.

The present paper reviews some of these theories, looks to the present facts on the distribution of income in the United States today. It concludes that the relevant U. S. income distribution shows far less skewness than we are accustomed to think, while such skewness as does exist is readily explained by the nature of our financial organization.

I. *A Review of Past Theories*

The apparent skewness of the income distribution, as demonstrated by Ammon and Pareto, is a triumphant first fact upon which a mass of economic literature has been erected. Explanations have been generously varied.

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A. *Transformation of the Normal Curve*

In one explanation the normal curve is transformed by Edgeworth's method: "the frequency curve of a variate is . . . 'translate' from a normal curve by the aid of an arbitrary function of its argument"[1] [55, p. 6] [5, p. 364] [33]. Essentially this method was used by H. L. Moore, R. Gibrat, D. G. Champernowne, M. Kalecki and others who add increments of income to an initial income distribution in proportion either (a) to the levels already achieved, or (b) to the excess in ability of given members of the distribution over the lowest (or median) member[2] [29, p. 89] [7] [8] [21] [39]. The difficulty with many such functions is a simple one: they do not fit the data. The original Pareto function described only a portion of the reported income distribution—a point initially recognized by Pareto but apparently later subject to underemphasis. Neither does the Gibrat formula, even as modified by Kalecki or Champernowne, fit the entire distribution at all well[3] [21, p. 169] [7, pp. 614-15].

What is particularly incomprehensible is the willingness of some analysts to deal with only whatever part of the income distribution fits the function—and then deduce serious economic consequences and deep social necessities. (The "virtual identity" of Pareto coefficients in many societies, to which many since Pareto have called attention, is a charming artistic observation, not a scientific one. The coefficient is capable of such limited variation that it would not appear much different in the ocular analysis of data for a Fourierist society, a communist or a capitalist one.) It is of more than passing interest that both a leading mathematician (W. Feller) and a distinguished economist (Pigou) have independently picked the same adjective when they describe Pareto's function, or the neccessity for it: "mysterious[4] [11, p. 418] [32, p. 650].

There exists, however, one method of "translating" the normal curve to derive a skewed distribution, and fit it comprehensively, that rests on a sounder and more penetrating economic analysis. It emphasizes the ownership of capital as the deciding element. Thus, Kapteyn states that

[1] Wicksell refers to the distribution of logarithms of variates developed by Fechner—presumably prior to Pareto's particular use.

[2] Cf. the work of Van der Vijk as noted in [10] and [39].

[3] Champernowne [7] develops formulae that fit better than Gibrat's [16], but if we apply them to the only set of data he presents that include low-income recipients—for the United States in 1947—his results are hardly satisfying. Thus even his 5-parameter formula gives estimates in error by 13 per cent or more for 4 of the 12 income intervals.

[4] Pigou refers scathingly to Pareto's distribution function "for the existence of which a mysterious necessity seems to have been discovered."

one of the principal causes of deviation in wealth is to be found in gains and losses of capital, these being "roughly proportional to the capital [already] possessed"[5] [23, p. 43]. This assertion, of course, forces the inquiry back one stage: how did the variations in capital themselves come about? The solution presumably is found in a factor emphasized by Pigou and Herbert Hoover—inheritance: the capital accumulation of many generations provides the basis for substantial current streams of property income[6] [32, p. 65] [19, p. 29]. These explanations are closely related to those of H. Bernadelli, R. S. G. Rutherford, and a brilliant unpublished study by Robert Solow—each of whom relates income in this year to income in the prior year[7] [2] [33] [34].

Now surely the biblical apothegm—to him who hath shall be given—is uncontestable, even when veiled in mathematical and analytic form. For despite the most random and extreme movements from year to year, persons do tend to maintain the same position within the income distribution. If we merely stop with last year's income, however, we are left with the question of how that pattern arose. If we continue the regress until we explain the income in a given year by the role of inheritance, what do we have? Certainly an explanation that might apply to some countries in the historic past.

But is it of substantive use for understanding how income is distributed in the United States? Suppose we take as an indirect indication of the power of inheritance in molding our income distribution the data we possess on the occupational ladder. Whether we look at the tremendous occupational shifts from immigrant parents to children of immigrants or to spot studies of such shifts in earlier decades, it is hard to develop a great role for inheritance in the past.[8] We have more recent evidence, though still very partial. It suggests that many in the upper occupational groups—where we would expect inherited property to be of most consequence—come from parents in the semiskilled, clerical and labor

[5] Kapteyn rejects Pearson's rather tautological analysis of skewness as originating "when the tendency to deviation of one side of the mean is unequal to the tendency to deviation on the other side" [23, p. 5]. He emphasizes instead that "causes independent of the size of the individual produce the normal curves, [while] causes dependent on this size produce skew curves" [23, p. 13].

[6] Herbert Hoover [19, p. 29] recommended inheritance taxes after the first world war "to thaw out frozen and inactive capital and the inherited control of the tools of production."

[7] Solow points out that in principle there is no reason why we need allow only for the income of the prior year. His model, however, and his empirical findings suggest that such other elements are unessential variates.

[8] Data on occupational shifts appear in E. P. Hutchinson [20]. The study by Taussig and Joslyn [37] is well known, while David A. Wells [54, p. 351-53] has some interesting data on the proposition of shirt sleeves to shirt sleeves in three generations.

groups.[9] And the income tax data for the nation as a whole show that current earnings play a dominant role even at high-income levels.[10]

B. *Two Overlapping Distributions*

An alternative approach contends that two independent factors or distributions are involved, each perhaps normal, but so overlapping that they create the characteristic skewed income distribution. In its simplest form, as in Boissevain's two-factor analysis, this analysis falls into difficulties because it must presume—without any particular warrant—that the factors are quite uncorrelated. Waxweiler [53], for example, proposes a completely independent factor on the supply side and one on the demand side. Unless we posit zero correlation, we are up against the difficulty that the joint distribution of two normally distributed variates will itself be normal. A distribution of intelligence quotients, for example, is normal even though it reflects half a dozen separate achievement distributions for different components of the standard I. Q. tests. Yet what warrant do we have for positing zero correlation to begin with? It is not particularly reasonable as an hypothesis. It is not especially heuristic as a premise.

Tinbergen, too, has distinguished two distributions. He points out that we are concerned not merely with the distribution of abilities to earn income but likewise with a distribution of preferences for income: "the valuations for the labour to be performed, on the one hand, and for the wages to be received, on the other hand."[11] He assumes the latter to be unequal, since "a person who has special ability for a job which requires an effort *s* will be prepared to accept a relatively lower wage than somebody who does not possess this ability" [39, pp. 199-200]. Let us concede that the resistless instinct of workmanship does persuade some expert members of the labor force to accept only average (or below-average) earnings rates. Let us go on to stipulate that it

[9] Cf. the occupational data in the six-city mobility study as reported in Herman Miller [27, p. 32].

[10] [51, Part I, Table 3] [49, Table 7]. From these sources it can be seen that if upper-income recipients with earnings *and* with other types of income earn no more than the average amount earned by others at the same income level, the residual contribution of their nonearned income to total income is relatively small. This residual must be increased somewhat to reflect the fact that in some instances earnings from self-employment may have been made possible in the first place only by inheritance of the business, or capital to buy the business. Yet even after making an allowance for such a factor, we would be hard put to maintain that inheritance is now a major factor in shaping the U.S. income distribution.

[11] [39, pp. 207, 198]. Tinbergen refers to the work of J. Van der Wijk, who develops a normal distribution not for income but "the logarithm of income less a certain minimum" —a procedure similar to that of H. L. Moore, Harold Davis, etc.

could persuade still others to do so if necessary to keep their skills burnished bright. But the evidence of today's market suggests that few persons do make such concessions: superior skills generally command superior rates. Where they fail to, it is not because their possessors willingly accept lower incomes [24, p. 79 and *passim*].[12]

As there is a taste for work, so there is a taste for risk—and this too has been invoked to explain the shape of the income distribution. This explanation starts from the common dichotomy between "those who prefer a secure though modest return . . . and those who play for big stakes and are willing to assume risk in proportion" [31, p. 289][13] As developed by Friedman the theory does not presume two groups of individuals but rather two sets of actions by every individual, one set having "results which are not accessible to redistribution," while the results of the other are [12, p. 288] [cf. 13] [cf. 2, p. 359]. Since the former set of actions involves participation in a lottery, no (sizable) negative income arises for any participant; the "fraction of winners is close to zero," but nevertheless positive; and a group of very high incomes appears. Such a process could readily account for the characteristic tail at the upper end of the income distribution [12, p. 289]. This well-developed and ingenious model is one of the very few that offers an explanation for the lack of a tail at the lower end of the income distribution. And it sorts out the key elements that must be included in a really useful analytic model.

But it does not necessarily bear closely on the actual distribution of income in the United States today. The person in our society with the greatest taste for risks is of course not the entrepreneur or millionaire but the small-income wage earner. It is the wage earner, and not the entrepreneur, who regularly bets against impossible odds—on policy lotteries, sweepstakes and football pools. Atkinson's fascinating data suggest that it is the small investor (more often than the wealthier) whose portfolio is dominated by low-rated, speculative stocks [1, pp. 125-26] [36]. Above all "a taste for risk" more obviously motivates the millions of wage earners who, in any year, throw up their jobs and uproot their families in the venturesome hope of bettering their incomes, than it does the prudent investor. For the latter hazards only a portion of his capital (after consulting his lawyer, broker and accountant), frequently seeking investments that have been guaranteed by a government agency, and relying on the consolation offered by government tax offset if the worst happens. Yet it is such investors with a taste for caution and

[12] More than one firm has adopted high-wage policies as a method of securing top-quality workers.

[13] Perlman refers the distinction back to Sombart's contrast between the guild aim of "a secure livelihood" and the businessman's "boundless desire to amass wealth" [31, p. 6].

the sure thing, who dominate the upper end of the income distribution.

However, most of these considerations are irrelevant: since neither the income receipts of the risk-taking wage earner nor those of the entrepreneur are "accessible to redistribution," the premises of the model are not met so far as concerns the present-day distribution of income in the United States.[14] Fruitful conclusions may yet flow from such premises. But there is, as yet, no substantial store of such conclusions.

It is on a related difficulty that Wicksell's (and latterly, Hayakawa's) analysis fails. Wicksell emphasized that earnings from business are "the sums of the profits in an (often great) number of transactions . . . the greater the risks . . . the greater eventual losses or returns" [55, p. 12] [18, p. 181].[15] Such theories could account for a symmetrical distribution, with entrepreneurs appearing in equal numbers at the lower end of the income scale and at the upper. They do not explain the actual one, with its positive skewness.

C. *Noncompeting Groups*

Of all the explanations of income inequality, the chief is probably the lack of mobility between social strata: the theory of noncompeting groups.[16] There exists, in Cairnes words, "a series of industrial layers . . . the several strata are, for all purposes of effective competition, practically isolated from each other" [6, p. 72].

What is most striking about this explanation is how distant its origins are. Can Adam Smith's bitter comments on social policy in the eighteenth century, when poor laws shackled the worker who sought to leave one shire for better wages in another, when free public education was nonexistent and when the secret ballot, trade unions and Chartist proposals were idle dreams—can such an analysis be used to explain income distribution in a society where public education is all but universal; where a host of incompletely differentiated occupations compose the labor force; and where substantial geographical mobility characterizes the labor market? How relevant to today's income distribution are Mill's once trenchant references to "hereditary distinctions of caste; each employment being chiefly recruited from the children of those

[14] At least in terms of this analysis; redistribution via the tax mechanism and government subsidy programs is another matter. The tax redistribution process helps destroy, not produce, incomes at the upper end of the distribution.

[15] Hayakawa [18] finds that the skewness derives from the sum of separate distributions for labor income, salary income, etc. For entrepreneurial income he specified that "positive skewness will be great"—but does not touch on the critical point of why it should exist, or why it should be great.

[16] J. E. Cairnes [6, Pt. I, Ch. 3]. Cf. the position of Benini and Gini as summarized in Hans Staehle's admirable study [35, p. 79] and Staehle's own exposition [35, p. 87]. Pareto's discussion of the conditions that preclude entry to *cadres* and classes in his later work [30, p. 2046] might be noted here.

already employed in it," with "the great body of labouring people" excluded from the most remunerative pursuits because they could acquire neither education nor training? [26, Bk. II, p. 459, and Bk. III, p. 544]. There are, unquestionably, monopoly elements which preclude free entrance into certain occupations.[17] The selection of talent from the population at large is limited by differential access to education, to training, to financing. Kind words do not an open society make. But for many decades the "career open to talents" has characterized U. S. society far better than the theory of noncompeting groups has characterized it, or than the latter characterized the English social order in which it was developed.

To pin such qualitative statements down a bit, let us attempt a crude numerical guess. We can assume that in eighteenth-century England the number of job changes during a year would have been extremely few—possibly less than 10 per cent. In contemporary America we can estimate that the ratio is well over 200 per cent. In 1954, for example, an average of 68 million persons were in the labor force at any one time. Yet roughly 100 million entrances into and exits from the labor force and shifts between farm and nonfarm jobs took place in that year; while we must add perhaps 70 million more job-changes within nonfarm or farm employments.[18]

Given such an enormous volume of labor market choices, entrances, exits, and shifts, our initial expectation would be that here, if anywhere, the classic conditions for producing a normal distribution must exist. Islands of noncompeting, of monopolistic and protected groups will certainly appear. Yet the broad pattern should be much like a normal distribution. A priori explanations in terms of noncompeting groups

[17] Walter Gellhorn [15, pp. 105-51] notes that "By 1952 more than 80 separate occupations, exclusive of owner businesses like restaurants and taxicab companies, had been licensed by state law." But however serious the principle involved, the proportion of the labor force concerned is clearly small.

[18] The annual average appears in [43, pp. 19, 35]. The shifts into and out of the labor force are shown in [44, Tables 17, 18, 19]. Estimates of shifts between jobs not already covered in these data were made by developing shift rates from two sets of data. For construction and trade, rates for 1949-52 were used as shown in a paper prepared by David Kaplan (based on Current Population Survey data) [22]. For all other employment, except manufacturing (i.e., primarily service and transport) the trade rate was used. For manufacturing employment OASI records on the percentage of covered workers changing industry code group from one quarter to the next were assumed to be more reliable. Rates were derived from these data by weighting rates for the separate age groups as reported in Isadore Blumen *et al.* [4, pp. 39-41, 67-73]. The estimate made here is a conservative one, since no allowance was made for job changes within agriculture not already accounted for by the labor force change data. (Since the number of persons with work experience in 1952 and 1954 was much the same, it was assumed that the gross change data for 1952, the last year published, applied equally well to 1954. The 120 million figure thus estimated was arbitrarily reduced to 100 million to compensate for the response variation present in these data.)

can hardly carry the conviction that they did when knowledge of the actual magnitudes of change in the U. S. labor market was far dimmer.

II. *The Ability of Individuals to Earn Income*

When so much expert analysis fails to give a reasonably coherent and applicable theory of why the income distribution in the United States today is as skewed as it is, we are well advised to return to first principles. What is it that we are seeking to measure when we deal with the income distribution? And how best shall we measure it?

We are concerned here with income as a measure of productivity rather than of welfare, hence with the ability of individuals to earn income under existing social and economic conditions. Conditions on the supply side have developed their talents for work, their abilities to invest, and have fired their desire for material goods. On the demand side still other factors have determined what places are available in the labor force, what skills and investments are requisite. Out of the interactions in the market come the rates of pay offered to ability and capital.

Given these market conditions, however, there can be no better measure of the distribution of the ability of individuals to earn income than the tautological one—namely the income they earn. But once the issue has been put in so flat-footed a fashion, it is clear that many of the income distributions used in developing the theories mentioned above are irrelevant.

1. *Tax Returns*. The distribution of persons filing tax returns has frequently been used, *faute de mieux,* for studying the distribution of income. The two are related, but only in a coarse and inconclusive fashion. For the tax population has no boundaries other than those set by the multitudinous provisions of this year's tax law and the vigor of this year's tax administrator. As the law is amended to cover more and more of the total population, the distribution inevitably changes. Even for the same year can we reasonably compare results for Norway (where tax returns cover 64 per cent of the population) with the Netherlands (where they cover 97 per cent) [40, p. 3]? And if we cannot, then how relevant is comparison of distributions and Pareto coefficients for the income tax populations of different countries at different times?[19] The scope of the income tax population is too narrow and arbitrary to carry us far if we seek to understand and measure the distribution of abilities to acquire income.

2. *Families*. The income distribution of families, or families and

[19] Pareto, of course, had no difficulty in doing so, even when the income tax population included corporate entities as well as persons.

single individuals, is a useful welfare indicator but it does not measure the ability of individuals to earn income. For that matter, it does not even measure the ability of families to earn income, since a family's income reflects not merely its ability to acquire income but the nature of its financial goals. Thus with fixed or slow-moving financial goals, the wives in many nonwhite families stopped working when the onset of the prosperity of the second world war increased their husbands' income [17]. But surely family earning-ability did not decrease. With a backward-sloping labor supply function growing incomes would at some point actually produce greater and greater income "inequality."

3. *All Earners.* The distribution of all income earners is no more satisfactory. Its chief limitation is that it includes both men and women. To exclude women from the income distribution studied would clearly have a major impact. But this must be done because a large proportion of all women with income do not attempt to utilize their full ability to earn income. Some women work only part of the year. Others work a full year but only part time. And many in either group do so because—given family obligations, their attitude toward work, the income goal they seek, etc.—they voluntarily limit their participation in the labor force. Under such circumstances what do we do if we include them in the income distribution being considered as a measure of abilities? Roughly the same thing as if we were to study the skewness in a distribution of intelligence quotients that combined scores achieved by persons who took only part of the examination together with scores for those who took the entire examination.[20] Fifty years of social legislation can change the Pareto coefficient less than an increase in Saturday jobs for women—if we are concentrating on the distribution of all earners or income recipients. (Possibly a meaningful distribution function could be developed for incomes of females who seek to work a full year but that is a separable problem, and a very complex one.)[21]

If we restrict our attention to the data for males, however, the same train of reasoning leads us to rule out two groups because they too do not attempt to exercise their full income-earning abilities. One group includes the youngsters, who attend school and at most work part time. The other is made up of older persons, many of whom are semiretired or completely retired. Perhaps the clearest demonstration that these groups must be excluded lies in a comparison of the trend in the earn-

[20] George Garvy [14, p. 40] has made the point very specifically. "The economic and social significance of part period workers is different from that of part time workers. Obviously $500 earned by a young man during the balance of the year on his first full time job after . . . graduation" differs from the same amount obtained in part-year employment of a family head who was in the labor force the entire year.

[21] The problem is analogous to that involved in developing tables of the work life for females.

ings and employment of males in these two age groups with that for the prime 35-44 group (Table 1):

TABLE 1—TRENDS IN EARNINGS AND EMPLOYMENT OF MALES, BY AGE

Age	Median Money Income of Males with Income			Per Cent of Males in Each Age Group Working a Full Year		
	1945	1956	Increase 1945–56	1939	1956	Change 1939–56
35–44	$2,473	$4,575	+$2,102	56	78	+22
14–19	389	412	+ 23	9	6	− 3
65 and over	1,225	1,421	+ 194	27	22	− 5

Source: Income data from Bur. Census, Current Population Reports, Ser. P-60, No. 2, Mar. 1948, Table 15, and No. 27, Apr. 1958, Table 18. Work experience data from Bur. Census, *1940 Census, Employment and Personal Characteristics*, Tables 1, 11, 31 and 33; and Current Population Reports, Ser. P-50, No. 77, Nov. 1957, Table B.

In a period when massive gains took place in wage rates and employment opportunities, full-time work by men in the prime 35-44 group rose greatly; their median income rose by roughly $2,100. Full-time work by youngsters and older workers, however, actually decreased; their gain in earnings consequently was no more than the mere rise in wage rates would have brought. Such relative stability for these two groups reflects one basic fact: their annual income measures primarily a level of incidental earnings, and not that of attempted full-year income.[22]

A third age group must likewise be excluded from the data for recent years—men 20-24. The reason for so doing is simply that a substantial number of these men serve in the armed forces during part (or all) of the year. Their annual income fails to measure what their income earning abilities would yield when not arbitrarily limited by government action.[23]

Of course if we could include in the income distribution being studied only the members of these age groups that did seek a full year's work (including the youngsters who enter the labor market early and untrained, the older workers who linger on unwanted for full-time work

[22] In studies by Dorfman and others, older workers have reported not working because they were "unable to work." One might, therefore, treat them as the institutional population is treated below. The meaning of such declarations, however, is as yet too obscure to warrant such classification. Some studies suggest that some older workers report "unable to work," meaning thereby "unable to secure work at wages and at skill levels to which they had been accustomed."

[23] Were recruiting from the civilian population a random matter, this contention would not apply, although there would still be a problem of part-year income receipt.

and who are paid at the lowest rates) a more realistic distribution would be developed. Should the data permit us to do so, however, it is certain that many of those to be included would not be at the lower end of the distribution. Moreover the entire adjustment could have only marginal impact: most persons in these age groups would continue to be omitted.

The principle on which we exclude such groups, however, reaches only to invariant characteristics such as age or sex. Some analysts, in an endeavor to develop a symmetrical distribution, have excluded part-year workers per se, the unemployed, those with zero income, those in certain occupations. There is little basis for such exclusions. For all we know about such groups is that they have not worked a full year or earned a sizable income. Neither fact demonstrates anything more than this—that they have brought to the market the least desired set of talents, and received the least employment and monetary reward in consequence. (We are not concerned here with native talent, use value, or wealth and illth: as a hack writer Oliver Goldsmith was too unreliable, as a bureaucrat Whistler was too volatile to enter the income distribution at a point where future generations might choose to put them.) The facts of low income and a short work-year per se give us no warrant for excluding any group from the income distribution. By the same token, what virtue lies in excluding a given occupational group to develop a distribution of Elysian symmetry?[24] For one of the basic mechanisms by which the economy rewards different earners differently is to allocate them to occupations, to industries, and to areas with characteristically different levels of income receipt.

One puzzling group is the institutional population. Though customarily excluded from income distributions, we cannot omit them here. Since society pays for their maintenance, we may most realistically define their income as though it were negative—since it is as clearly a withdrawal from other incomes (or capital) as are the losses of entrepreneurs.[25] Therefore, instead of classifying them with the zero-income group—a reasonable alternative—their limitations of physical and mental qualities seem best reflected by classifying them as having negative incomes.[26]

[24] For a convenient and lucid presentation of the data by employment status and occupation, see Herman Miller [27, esp. Ch. 3] [28]. For an effective review of the wage rate data in one area see Frederic Meyers [25].

[25] Ammon implied this classification for "beggars, inmates of institutions" and those "who generally do not receive any income of their own, and yet are being maintained" [35, p. 77].

[26] The 1950 Census report [42, Table 3] gives us the basis for estimating the number of males in the 25-64 age group. This total was raised to 1954 levels, but reduced to exclude persons in correctional institutions [42, Table 4] on the analogy of the armed forces, their

III. *Incomes of Males Aged 25-64*

Suppose, then, in our attempt to measure the ability of individuals to earn income under existing social and economic conditions we pass on from income distributions for taxpayers, families, individuals, to that for all males, and thence to the male distribution exclusive of those age groups in which the typical person does not attempt to earn a full year's

TABLE 2—MONEY-INCOME DISTRIBUTION, CIVILIAN MALES, 1951
(Per cent distribution)

	All Males	Males Age 25–64	
Under $1,000	17.5	8.7	8.4
$1,000–1,999	13.9	11.8	11.4
2,000–2,999	19.4	20.9	20.2
3,000–3,999	22.6	26.3	25.4
4,000–4,999	12.7	15.5	15.0
5,000–5,999	6.4	7.8	7.5
6,000–9,999	5.4	6.9	6.7
10,000 and over	1.9	2.1	2.0
Total with income	100.0	100.0	96.6
Without income			1.9
Institutional			1.5
Grand Total (civilian)			100.0

Source: Bur. Census, Current Population Reports, Ser. P-60, No. 11, May 1953, Table 3; and 1950 Census, *Institutional Population*, Table 3.

income. We are left with the distribution of Figure 1 and Table 2 for the 37 million males aged 25-64.[27] This distribution covers a large, endlessly varied set of persons—the fully employed and those who secure

income, too, being limited by society. One could, of course, make an argument to the effect that the very fact of their being in correctional institutions was a reflection of their inability to earn income under current social arrangements. However, the empirical effect on the income distribution of either choice is minute.

[27] Data on 1951 incomes from Bureau of the Census [48, Table 3]. These and other survey data have certain biases for which extensive adjustment has been made by the Office of Business Economics [52, pp. 27ff.]. (A comparison between family income distributions in Table 1 of the parallel Census report for 1950 [47] and the adjusted OBE family distribution in Table 19 of the latter study indicates that the major adjustments are in the under-$1,000 income group. These adjustments were made on an over-all basis. One cannot therefore demonstrate, but only assume, that the disproportionate dominance in the low-income group of under-20 and over-65 persons would have accounted disproprotionately for the bias in that group. Hence, the restriction of present data to the 25-64 age group would have removed a major share of the differential bias present in unadjusted data.)

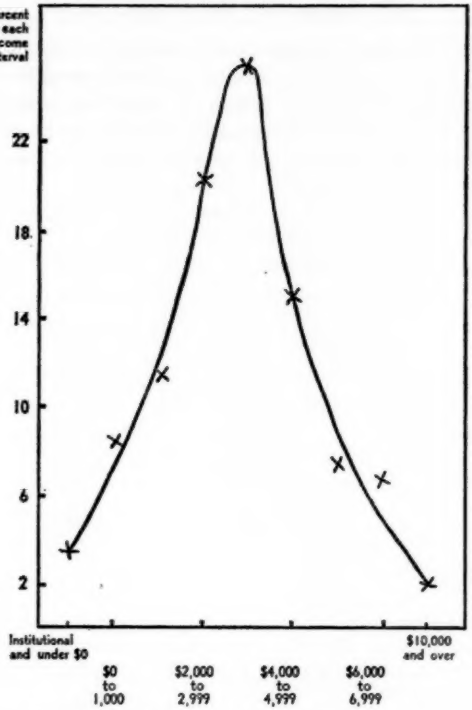

FIGURE 1. MONEY INCOME IN 1951: MALES 25-64.

only part-year work; the quick and the halt; the brilliant and the slow; those with wealth beyond the dreams of avarice and those who live on society's bounty. Nevertheless, the joint distribution of all these persons is clearly much more symmetrical than the typical Pareto curve. One hardly has to invoke the shades of noncompeting groups, inheritance, preference for risk, etc. to explain this pattern. A log-normal function is not so obviously apposite. On the other hand, distinct skewness still appears. Can we explain it?

IV. *Credit Rationing and Labor Demand*

In Table 3 there appear estimates for two major groups that compose the 25-64 distribution—those who are self-employed, and those who are not. Since the time of Solon (and certainly since that of Wicksell) it has been clear that the self-employed contribute disproportionately to the skewness of the income distribution. These data reaffirm this gen-

TABLE 3—MONEY INCOME DISTRIBUTION OF MALES, AGE 25–64 IN 1951
(Percent distribution by class of worker)

	Total	Employees	Self-Employed
Under 1,000 Loss	0.7	0.2	1.8
Under 1,000 1–999	7.7	5.0	13.9
1–1,999	11.4	11.9	10.8
2–2,999	20.2	23.1	14.5
3–3,999	25.4	29.8	16.5
4–4,999	15.0	16.4	12.4
5–6,999	7.5	6.7	9.5
7–9,999	6.7	3.2	14.7
10,000 and over	2.0	.3	5.8
Total with income	96.6	96.6	100.0
Without income	1.9	1.9	—
Institutional	1.5	1.5	—
Grand total	100.0	100.0	100.0

Source: Bur. Census, unpublished data. The Census data relate not to class-of-worker but to occupational group. Farmers, professionals, managers and proprietors were here combined as an approximation of the self-employed group. The resultant inclusion of wage earners is probably most important for the $1,000–$3,000 income level but would affect the contrast of the two distributions very little.

erally accepted point. But if we examine them closely, they do more.

The wage-earner group itself is merely composed of those persons who were wage earners as of the week of enumeration. During the year an average of 400,000 wage earners a month entered self-employment and almost as many self-employed persons entered the wage-earner group.[28] The presence of this component may have been a factor in shaping the distribution for employees, but when we turn to the distribution for the self-employed it is clear that it is this latter group that is the source of asymmetry. Customary explanations of the skewness in the income distribution concentrate on explaining the presence of the tail at the upper portion of the distribution. But it is clear that the self-employed largely account for this portion. Since the self-employed can lose as well as make money, however, why should not there also be a lower tail of equal length, also produced by the presence of the self-employed?

Let us consider a simple model of the supply and demand for labor

[28] Estimate based on unpublished data from the Census Bureau's Current Population Survey. These gross-change figures, as others, are peculiarly subject to response variation. However, for present purposes it makes little difference whether the true figure is markedly smaller.

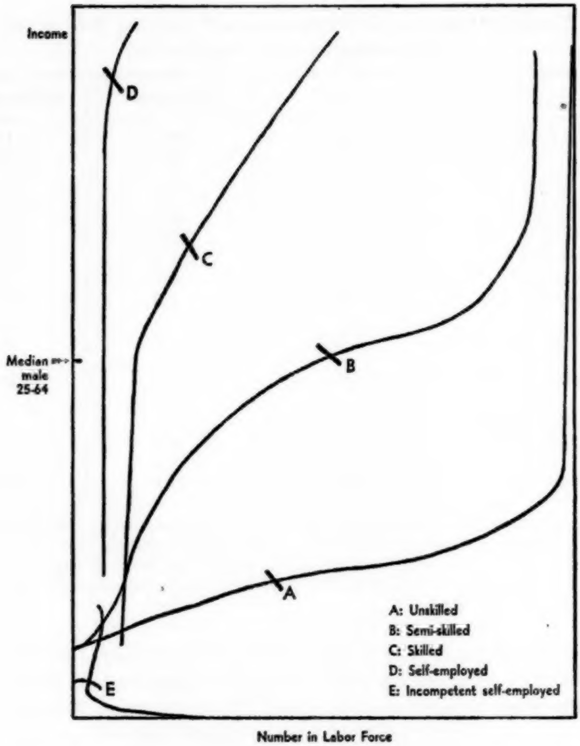

FIGURE 2. SUPPLY AND DEMAND SCHEDULES BY OCCUPATION GROUP

in different occupations as a means to answering this question. Figure 2 portrays supply schedules for selected occupational groups—together with the portions of the relevant demand schedules where they intersect the appropriate supply schedules. The income of the median male is arbitrarily set to equal that of the occupation group in which a majority of males are engaged. (In fact the median for male semiskilled employees in the United States is much the same as that for full-time males in the labor force.) Averages for the other occupation groups are indicated. These then translate into a symmetrical distribution if the rates for one group are smoothed into those for the other. This smoothing occurs in practice for two reasons. First, in reality not 5 groups but hundreds (possibly thousands) of clearly differentiated occupations, with their several wage rates, exist. Second, the market even for one of these occupations is hardly perfect. Ignorance of prevailing rates exists

among employers as well as employees, the momentary exigencies of supply and demand in the process of reaching toward a never-achieved goal of full equilibrium result in a stochastic process, producing a spectrum of rates paid in even the most specific, keenly differentiated occupation. The conversion of rates into income is in turn affected by illness, personal preference, irregularities in the production process and so on.

That the numbers and prices that clear the markets translate into a smooth distribution can therefore be seen as reasonable. But how does this symmetrical distribution relate to the actual one? Here one may refer to a key dialogue between Holmes and the Inspector:

> "Is there any point to which you would wish to call my attention?
> "To the curious incident of the dog in the night-time.
> "The dog did nothing in the night-time.
> "That was the curious incident, remarked Sherlock Holmes."

The most noteworthy aspect of the usual income distribution is not that it has one long tail—but that it does not have two. The mere presence of one tail is no more sufficient to produce asymmetry than is the presence of one leg sufficient to make a man one-legged. The symmetry that derives from the schedules in Figure 2 comes about because of the presence of occupation group E. This group is composed of the incompetent self-employed—those capable of losing large sums of money as entrepreneurs. A rare combination of persuasiveness, avarice, and recklessness is needed. How widespread is it? Perhaps an indication appears in the income tax figures for 1956, which report more than 3 million persons with incomes over $10,000—but less than 30,000 with an adjusted gross loss from business of over $10,000 [51, Tables 1, 6]. But surely there must be more than 1 in every 100 would-be entrepreneurs sufficiently incompetent to lose $10,000. If so, and if there is no reason to assume (as is frequently done) that the income distribution must begin at $0, why does the real distribution lack a significant number with negative incomes? The answer of course is that, while there may be a great potential supply, the demand is almost minute. And the key lies in our credit system.

Who can be a Samuel Insull, Ivar Kreuger or John Law on his own savings? As it generally requires credit to acquire substantial incomes so it requires credit to achieve spectacular bankruptcies. By extending credit to those with great ability to make money the credit agencies help produce one end of the income distribution. By refusing credit to those with great abilities to lose money, they truncate the other end.

V. *Some Conclusions*

Incomes that are relatively high compared to typical incomes appear in every country in the world. Even in socialist societies, theory has

caught up with practice in arguing the necessity for distinctions in earning rates in order to encourage workers to train for the more skilled occupations and to reward higher productivity.[29] It is not so much the higher incomes which require explanation, therefore, as the lower ones —in this case the negative ones.

The range from median to top income will vary from sheikdom to sheikdom, from capitalist to communist state. But all appear to have high incomes relative to the median while none have any considerable number of negative incomes. Credit availability plays a sufficient part in facilitating the receipt of high incomes in some countries, a necessary part in others—but in all countries with private credit systems credit rationing appears to be a major factor in preventing large negative incomes. As such it has a decisive role in producing the skewness in the income distribution.

What, in summary, has the preceding review of income distribution theories covered? It has not attempted to touch on the welfare aspects of income distribution. Income may be normally distributed and yet a given society may decide that all incomes are "too low" because productive potentials have not been fully tapped, or that incomes are "unfairly distributed" because a tax-subsidy program could make many persons "better off" and only a few "worse off." On such problems welfare economics may yet be able to say a good deal but this is not our present concern.

The present subject has been whether income, as a measure of the productivity of income recipients, is normally distributed in the United States today. In examining this question we have concluded that the customary conclusion (or premise?) that this income distribution is highly skewed tends to derive from the study of nonrelevant distributions—such as those for taxpayers, for families, for all individuals. We have contended that a relevant (and most available) distribution for this purpose is that for males 25-64—and that such a distribution is remarkably like the normal gaussian, remarkably unlike the usual skewed distribution. In explaining the skewness that does nonetheless appear in some measure in that distribution it has been suggested that we are really confronted by the impact of credit rationing in truncating a normal distribution. By and large, credit agencies succeed in denying credit to persons capable of losing large sums of money (and thus contributing a long lower tail to the distribution) while affording credit to those who use such leverage to acquire large income.

Few theories that have been developed fail to help in understanding

[29] It is interesting to find as presumably accepted doctrine a statement by Oscar Lange indicating "la nécessité d'une differentiation des salaires, en vue de stimuler la productivité du travail," quoted in [3, p. 793].

how income is in fact distributed. Inheritance may play a greater role in other societies, but it is certainly a factor in our own. Noncompeting groups and the ability of those with finer educations and preferred social origins to acquire larger incomes are factors of real force in all societies. The taste for risk, a willingness to grasp the hazard of new fortunes is a further element, certainly in the United States. But if we seek to understand the shape of the income distribution relevant to the measurement of productivity (and not of welfare) it would appear unnecessary to invoke these other elements. The shape is reasonably well defined as a normal distribution, truncated by credit rationing.

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THE ACCELERATION PRINCIPLE: DEPARTMENT STORE INVENTORIES, 1920-1956

By NEWTON Y. ROBINSON*

The primary purpose of this investigation was a partial testing of an adaptation of the multiplier-accelerator type of theory[1] to an explanation of the minor business cycles which are frequently considered to be "inventory cycles." The basic assumptions of this adapted theory are (1) that changes in the level of income during the periods immediately preceding period *n* will, on the average, lead to a substantial amount of induced investment[2] during period *n*, and (2) that consumption during period *n* is largely the result of income in several previous periods. This study represents only a partial testing because it does not test the relationship between consumption and income and because it measures the amount of induced inventory investment in only one segment of the economy.

I. *Method*

At the time this study was begun, the amount of monthly inventory data available was severely limited, a condition which is gradually being corrected. This project, consequently, was restricted to an intensive study of the substantial body of excellent seasonally adjusted monthly data on department store stocks and sales.[3] The statistical method used

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[1] The theory being tested is essentially an adaptation of Hicks' general case. See [1, pp. 74-81]. The principal adaptation consists of using a short fixed period (one month) in order to reveal inventory fluctuations more completely.

[2] The exact amount necessary for an expanding cycle will depend on the consumption function and the lag pattern of induced investment. Hicks argues that the total investment coefficient (i.e., the ratio of induced investment to change in income) must at least be greater than 1 [1, p. 78].

[3] The data used were slightly more precise than those published in the *Federal Reserve Bulletin*. They were supplied through the courtesy of O. K. Thompson of the Division of Research and Statistics of the Board of Governors of the Federal Reserve System.

was that of multiple regression. The regression of change in inventories for period n on changes in sales in several antecedent periods was calculated by the method of least squares.

It is possible that department store officials are more affected by "current dollar" figures than the "real" quantities underlying them. Use of undeflated data, however, could yield misleading results. If, for example, there were no change in real stocks or real sales but price changes did occur, an observed correlation might be found in the undeflated data which was due solely to the fact that changes in prices caused simultaneous changes in the current dollar figures for both stocks and sales. To eliminate this possible source of bias, deflators were constructed from two components of the Bureau of Labor Statistics Consumer Price Index: apparel prices, given a weight of 2; and housefurnishing prices, given a weight of 1.

To prevent bias due to the fact that both department store sales and inventories have an upward trend, changes in deflated sales and inventories were respectively expressed as percentages of a twelve-months moving average of deflated sales and inventories. The moving average computed for any given month was the average of the month in question and the 11 immediately preceding months.

The use of percentage differences also guards against another important possible source of misleading results. For example, inventory investment during any given month will be affected by sales in each of several preceding months. If sales for period n-1 are included as one independent variable and sales for period n-2 are also included as another independent variable, a quite high correlation would exist between these two independent variables. Where high correlations exist between independent variables, the system becomes unstable (i.e., small changes in the input data cause large changes in the regression coefficients). Use of percentage changes in sales, rather than sales themselves, substantially reduced the correlation between those independent variables, resulting in a stable system. Evidence of the stability of the system was obtained from a study of the inverted correlation matrices. Since none of the elements of the inverse were unduly large, the system was stable.[4]

Another statistical technique which should be explained is the use of absolute values (i. e., numerical values without regard to sign) of per cent changes in sales to test the hypothesis that upward changes have a different effect from downward changes in sales. If the absolute values

[4] Indicated orally by Mark Robinson who quotes John von Neumann and H. H. Goldstine [2]. Actually, a number of the multiple correlations performed in the course of this study were considered to be unstable. The results of such multiple correlations are neither presented nor discussed here.

of changes in sales for a certain lag show a statistically significant positive *partial* correlation with inventory changes, when algebraic changes in sales for the same lag are included as another variable and also show a significant positive partial correlation, then the evidence indicates that an increase in sales for the given period has a greater effect on inventory change than does a decrease.[5]

In addition to the variables of primary interest, other variables were included in the multiple regression to investigate their effect upon inventory change. These additional variables included changes in wholesale prices (expressed as a percent of the twelve-months moving average of wholesale prices) for several different lags, inventory changes (expressed as a per cent of the twelve-months moving average of inventory change) for several different lags, the prime rate of interest and change in the prime rate of interest. Other variables that were included did not yield results of much value.[6] Since several series were included for different lags, each lag constituting a separate independent variable, a total of 33 variables were included.

The multiple regression program permitted the elimination of variables which proved undesirable. The last phase of the multiple regression (matrix inversion) could then be performed without those variables. Consequently, a substantial number of multiple correlations were performed to test the effect of eliminating or not eliminating certain variables or groups of variables, or to eliminate variables which were not statistically significant or which caused instability.

The observations were broken into two periods which eliminate the months that were most disturbed by the second world war and the Korean war. The first and most important of these periods was the interwar period, March 1920 through July 1941. October 1948 through June 1950, and November 1951 through March 1956 were included in the postwar period. All the observations included in the interwar or postwar periods were included in what is called the combined period. Thus we have three sets of results: for the interwar period, for the postwar period, and for the combined period.

[5] If both algebraic sales-changes for period n-2 and the absolute values of sales-changes for period n-2 are included, we have two regression coefficients for period n-2. If we assume that the regression coefficient of the algebraic changes in sales for period n-2 is .5 and the regressional coefficient of the absolute values of changes in sales for period n-2 is .2, an increase in sales of 1 per cent during period n-2 will, on the average, cause an increase in inventories during period n of .5 per cent plus .2 per cent, or .7 per cent. On the other hand, a sales decrease of 1 per cent during period n-2 would cause, on the average, a decrease in inventories of .5 per cent plus an increase of .2 per cent, or a net decrease in inventories of .3 per cent.

[6] These were deflated sales for period n-2, deflated stocks for period n-2, and the average stocks/sales ratio (average of period n-1, n-2 and n-3).

II. *Results and Their Interpretation*

By far the most interesting and important results were the estimates of the amount of induced investment in department store inventories associated with changes in department store sales. Since a considerable number of multiple correlations were performed, there is a question as to which provides the best estimate of the amount of induced investment. For the purpose of testing multiplier-accelerator theories, it is not necessary to determine whether changes in sales directly cause changes in inventories or whether changes in sales are associated with changes in some other variables which cause changes in inventories. Hence, it is not merely unnecessary but actually undesirable to include any variables other than the dependent variable per cent change in inventories during period *n*, and per cent changes in sales in various periods. Inclusion of any other variables would tend to distort measurements of the extent to which changes in sales are followed by changes in inventories.[7]

Great care must be taken in interpreting the regression coefficients. Since the regression equation has been computed as an expression of the relationship of per cent change in deflated stocks and per cent change in deflated sales, a regression coefficient of .3 for changes in sales during period n-2 indicates that an increase in sales of 1 per cent during period n-2 is, on the average, associated with an increase in inventories of .3 per cent during period n. The effect on the economy, however, is more clearly measured by the dollars-and-cents change in inventories (using constant dollars) than by the percentage change. To estimate the dollars-and-cents change in inventories associated with a change in sales of $1.00 it is necessary to know the relative sizes of stocks and sales.

To meet this need, the average stock/sales ratio for the period has been computed. This ratio, however, differs from the usual type of

[7] A possible exception would be the existence of accidental correlation. For example, if x and y are two independent variables, both causal factors with respect to the dependent variable, and are accidentally (not functionally) correlated, the omission of either x or y will improperly cause the other to act as a proxy variable for the omitted one. The proper safeguard against such an occurrence is testing by means of confidence limits. Due to the large number of observations included in both the interwar and combined periods, the one per cent confidence limits are quite small. This means that any accidental correlation between x and y is almost surely so small that they are extremely poor proxy variables for each other. Consequently, the likelihood of obtaining misleading results from this cause is quite limited.

On the other hand, if y is functionally related to the dependent variable but causation runs from the dependent variable to y, inclusion of y will almost surely distort the relation between x and the dependent variable. In the current study this represents a far greater danger. Furthermore, if x causes y, while y is significantly correlated to the dependent variable solely for this reason, inclusion of y will tend to rob x of some of its true effect. This, again, seems far more serious than the first-mentioned danger.

stocks/sales ratio in which both stocks and sales are valued at retail prices. The true amount of investment by department stores must be measured by what department stores have spent on laying in inventories, rather than by the retail value of those inventories. Consequently, it was necessary to compute a rather usual type of stocks/sales ratio, in which stocks are expressed at cost and sales are, of course, expressed in actual retail prices. The average stocks/sales ratios obtained in this manner for the interwar, postwar and combined periods were 2, 1.7 and 1.9, respectively.

These stocks/sales ratios were used in the following manner. The ratio of 2 for the interwar period indicates that a 1 per cent change in inventories involved a change of 2 times as many dollars as a 1 per cent change in sales. The regression coefficient, for any one of the variables expressed as percentage changes, indicates the percentage change in inventories associated with a 1 per cent change in that variable. Consequently, a regression coefficient of .3 for proportional change in sales during period n-2 indicates that a change in sales of x dollars during period n-2 is associated with an increase in inventories, during period n, of .3 times 2 times x dollars or .6 times x dollars. The product of this multiplication (the regression coefficient times the stocks/sales ratio) will be called the acceleration coefficient. The sum of the acceleration coefficients for the antecedent periods which are found to be statistically significant will be called the total acceleration coefficient.[8]

We have used the total acceleration coefficient to measure the sum of the effects of changes in sales over several past periods on inventory investment during period n. The total acceleration coefficient, however, also measures the sum of the effects of changes in sales during period n on inventory investment during various future periods. For example, the month of January is period n-1 when n is February, n-2 when n is March, n-3 when n is April, etc. Consequently, the estimated total effect of changes in sales during January, on inventory changes during the following months, can be found from the sum of the regression coefficients for periods n-1, n-2, n-3, etc.

More important than the acceleration coefficients are the estimates of the investment and total investment coefficients. The invesment coefficients were obtained in the following manner. The regression coefficients (which relate percentage changes in inventories to percentage changes in sales) were multiplied by the ratio of stocks (at cost) to value-added by department stores. The product represents the ratio of inventory change to change in value-added. Since value-added is the

[8] These coefficients are not the same as the Hicksian investment or total investment coefficients. They are presented here only because the acceleration principle is frequently couched in terms of the relationship between investment and sales.

income created by department stores, this is also the ratio of inventory investment to change in income created by department stores. The sum of the investment coefficients which are found to be statistically significant will be called the total investment coefficient.

The relationship between the total acceleration coefficient and this total investment coefficient is not complicated. The total acceleration coefficient is the ratio between inventory change and change in sales. The total investment coefficient is the ratio between inventory change and change in value-added. Consequently it is possible to convert the total acceleration coefficient into the total investment coefficient by multiplying it by the ratio of sales to value-added. This is feasible since value-added is a stable percentage of sales. Since our adapted multiplier-accelerator theory is concerned with the relationship between investment and changes in income or production, the total investment coefficient is more convenient than the total acceleration coefficient in testing that theory.

As mentioned above, regression coefficients which are not statistically significant are excluded from the total acceleration and total investment coefficients. Unfortunately, however, tests of statistical significance rely on the assumption of a known probability distribution of the quantity whose significance is being tested. Since these probability distributions were not known, the tests of significance are questionable. Questionable tests of significance, however, are probably better than none at all.

Since no preferable assumption was available, the tests of significance were based on the assumption of normal distribution. The standard error of each regression coefficient was calculated. Under the assumption of normal distribution, each regression coefficient is significant at the 1 per cent level if at least 2.58 times as large as its standard error. Regression coefficients of the sales-change variables which were not significant by this test were not included in the computations of the total acceleration or total investment coefficients.[9] Variables which consist of the absolute values of percentage changes in sales were also not included.

The results of the multiple correlations considered most suitable for measuring the amount of induced investment are given in Table 1. Variables other than percentage inventory change for period n and percentage changes in sales for several periods were not included in these particular multiple correlations.

[9] Due to the fact that the multiple regression program permits the elimination of only a limited number of variables, elimination of any of the variables included in the multiple correlations presented in Table 1 would require a substantial amount of recomputation. Elimination of other sales-change variables (i.e., other than those included in Table 1) which were not statistically significant had very little effect upon the regression coefficients of the remaining sales-change variables.

TABLE 1—INDUCED INVESTMENT IN DEPARTMENT STORE INVENTORIES

Variable[a]	Interwar Period (March 1920 through July 1941)				Postwar Period (October 1948 through June 1950 and November 1951 through March 1956)				Combined Period (Combined Interwar and Postwar periods)			
	Regression Coefficient	Standard Error of Regression Coefficient	Acceleration Coefficient	Investment Coefficient	Regression Coefficient	Standard Error of Regression Coefficient	Acceleration Coefficient	Investment Coefficient	Regression Coefficient	Standard Error of Regression Coefficient	Acceleration Coefficient	Investment Coefficient
ΔI_n	1.000	—	—	—	1.000	—	—	—	1.000	—	—	—
ΔS_n	0.048	0.027	0.096[b]	0.245[b]	—[e]	—	—	—	0.033	0.025	0.063[b]	0.162[b]
ΔS_{n-1}	0.222	0.029	0.444	1.132	0.285	0.060	0.485	1.254	0.236	0.027	0.448	1.156
ΔS_{n-2}	0.279	0.028	0.558	1.423	0.213	0.072	0.362	0.937	0.271	0.026	0.515	1.328
ΔS_{n-3}	0.153	0.029	0.306	0.780	0.090	0.073	0.153[b]	0.396[b]	0.148	0.027	0.281	0.725
ΔS_{n-4}	0.128	0.029	0.256	0.653	0.062	0.071	0.105[b]	0.273[b]	0.113	0.026	0.215	0.554
ΔS_{n-5}	0.071	0.027	0.142	0.362	0.209	0.063	0.355	0.920	0.084	0.024	0.160	0.412
$(\Delta S_{n-6}+\Delta S_{n-7}+\Delta S_{n-8})$	—[e]	—	—	—	0.130	0.047	0.221[d]	0.572[d]	—[e]	—	—	—
$\lvert(\Delta S_{n-1}+\Delta S_{n-2})\rvert$	−0.081	0.035	−0.162[c]	−0.413[c]	−0.256	0.085	−0.435[c]	−1.126[c]	−0.101	0.032	−0.192[c]	−0.495[c]
$\lvert(\Delta S_{n-3}+\Delta S_{n-4})\rvert$	0.063	0.035	0.126[c]	0.321[c]	0.169	0.085	0.287[c]	0.744[c]	0.072	0.032	0.137[c]	0.353[c]
Total			1.706	4.350			1.865	4.827			1.619	4.175
$\bar{R}$	0.611	(0.040)[f]			0.576	(0.083)[f]			0.587	(0.037)[f]		

See facing page for explanation of symbols.

III. *Conclusions*

Little confidence is placed in the results for the postwar periods, since the small number of observations causes the standard errors of the regression coefficients to be comparatively large and since the disturbed nature of the times might logically be expected to cause abnormal results. Since results for the postwar period are somewhat different from the interwar results, and this may be due to a fundamental change in the relationships between the variables or to the disturbed nature of the postwar period, there are possible objections to combining the interwar and postwar periods. Consequently, unless otherwise stated, only results obtained for the interwar period are discussed below. Results for the combined period, however, are very similar to the interwar period, with only minor exceptions.

1. *Induced investment.* The estimate of 4.35 for the total investment coefficient for the interwar period means that a change of $1 in the

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[a] Symbols used in this table are as follows:
n is the period;
P is the retail price deflator centered in the middle of the month;
p is the retail price deflator calculated for the end of the month;
S is the seasonally adjusted index of sales;
I is the seasonally adjusted index of stock (inventories);

$$\Delta I_n = \frac{100\left(\dfrac{I_n}{p_n} - \dfrac{I_{n-1}}{p_{n-1}}\right)}{\dfrac{1}{12}\left(\dfrac{I_n}{p_n} + \cdots + \dfrac{I_{n-11}}{p_{n-11}}\right)};$$

$$\Delta S_n = \frac{100\left(\dfrac{S_n}{P_n} - \dfrac{S_{n-1}}{P_{n-1}}\right)}{\dfrac{1}{12}\left(\dfrac{S_n}{P_n} + \cdots + \dfrac{S_{n-11}}{P_{n-11}}\right)};$$

$|(\Delta S_{n-1}+\Delta S_{n-2})|$ is the absolute value (i.e., numerical value without regard to sign) of the sum of ΔS_{n-1} plus ΔS_{n-2}.

[b] Variables whose regression coefficients are less than 2.58 times their standard errors are not included in the total acceleration and total investment coefficients.

[c] Variable which consist of the absolute values of changes in sales are not included in the computation of the total acceleration and total investment coefficients.

[d] Since the coefficient for this variable is the average coefficient applying to 3 different periods it is counted 3 times in computing the total acceleration and total investment coefficients.

[e] These variables were so highly insignificant in other multiple correlations that they were eliminated in the multiple correlations presented here.

[f] Standard error of $\bar{R}$.

Note: The regression coefficients and their standard errors, which were calculated to 8 significant figures, are here rounded to 3 decimal places.

level of monthly income created by department stores led to an average change of \$4.35 in department store inventories (valued at cost). This estimate is sufficiently large to lend considerable support to the adaptation of the multiplier-accelerator type of theory to the explanation of minor business cycles. There is no reason to assume, however, that the coefficients applying to other sectors of the economy were equally large.

The estimated total investment coefficient for the postwar period of 4.83 is similar to the estimate for the interwar period. The average lag in the adjustment of inventories to changes in sales, however, was considerably longer. Although the postwar period used in this study does not include times of extensive shortages after the second world war or during the early part of the Korean war, it remains possible that minor shortages of goods (i.e., partial contact with the "ceiling" in Hicks' terminology [1, Ch. 10]) could be responsible for the greater average lag.

Results obtained for the variables which consisted of the absolute values of changes in sales indicate that the average lag was greater during expansions than during contractions. This suggests the possibility that partial contact with the ceiling was responsible for some of the delay during expansions. The fact that the difference between expansions and contractions was greatest during the postwar periods, when partial contact with the ceiling seems most likely to have been a significant factor, tends to support this belief.

2. *Forecasting by store officials.* Another point suggested by the sales-change variables is that department store officials were rather good at forecasting changes in sales. This follows from the fact that the partial correlation between sales change for period n and inventory change for the same period was generally positive. Negative partial correlations might have been expected, since an unforeseen increase in sales tends to cause a temporary drop in inventories.

3. *Other variables.* A substantial number of variables, not included in Table 1, were used in other multiple correlations performed in the course of this study. The conclusions obtained from these other variables are as follows:

a. Past inventory changes. Past inventory change (expressed as a percentage of the 12-months moving average of deflated inventories) was included for several different lags, each lag constituting a separate independent variable. Results appear to indicate that past inventory change tended to reduce or forestall changes in the same direction and to reinforce any influences causing a change in the opposite direction.[10]

[10] An exception was percentage inventory change lagged by only one month, which showed significant positive partial correlations. Due to the delays involved in a decision to increase inventories as well as the uncertain length of these delays, a decision to increase

The measured effect was so slight, however, that in only one multiple correlation was it significant at the 1 per cent level and then only barely so. The maximum negative partial correlation was found for a lag of about 7 months.[11]

b. The rate of interest. The rate of interest on prime commercial paper, 4- to 6-months, lagged by one month, was included as an independent variable in several multiple correlations. The simple and partial correlations obtained were uniformly positive but of such small size as not to be statistically significant,[12] which contradicts the belief that high interest rates significantly deter inventory investment or that low interest rates favor inventory investment.

Change in the prime rate of interest between periods *n*-1 and *n*-7 was included as an independent variable in some of the multiple correlations which also included the prime rate. In every case, results did not approach statistical significance.

c. Wholesale prices. An index of wholesale prices was calculated from two components of the Bureau of Labor Statistics Wholesale Price Index: textile products prices, given a weight of 2; and house furnishing goods prices, given a weight of 1. Changes in this index, expressed as a percentage of the 12-months moving average of the index, were included for several different lags, each lag constituting a different independent variable. The only variable of this group which attained statistical significance was percentage change in wholesale prices for period *n-2*. This variable showed a positive partial correlation which was only barely significant at the 5 per cent level in only one of the numerous multiple correlations in which it was included. Consequently, it appears that changes in wholesale prices were not an important factor affecting inventory investment by department stores.

d. The remaining variables. No significant conclusions were obtained for the remaining variables: deflated sales for period *n-2*; deflated stocks for period *n-2*; and the average stocks/sales ratio for periods *n*-1, *n-2*, and *n*-3. This, however, is not a wholly negative conclusion.

inventories during period *n*-1 will usually also be a decision to increase inventories during period *n*. If this is true, the positive partial correlation does nothing to show what conditions were associated with decision to increase inventories, but instead, tends to reduce the observed effect of the variables which led to this decision.

[11] More precisely, the maximum negative partial correlation was found for a single variable which consisted of the sums of per cent inventory changes in periods *n*-6, *n*-7 and *n*-8.

[12] In two multiple correlations the prime rate showed a positive partial correlation which was significant at the 1 per cent level. In both cases the multiple correlation included another independent variable which was so highly correlated with the prime rate as to create a condition of instability when both are included in the same multiple correlation. Consequently, the only two cases in which the prime rate was statistically significant should be disregarded.

The inability to find any variables other than past changes in sales which importantly influenced inventory investment by department stores tends to support the belief that this type of investment was primarily induced by changes in sales.

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CANADA'S ECONOMIC PROSPECTS

A Review Article

By SIMON KUZNETS*

This paper is a review, necessarily selective and sketchy, of the *Final Report* and supporting monographs that are the published product of the Royal Commission on Canada's Economic Prospects. The task of the Commission, in the language of the relevant Privy Council order of June 1955, was to "inquire into and report upon the long-term prospects of the Canadian economy, that is to say, upon the probable economic development of Canada and the problems to which such development appears likely to give rise . . .".[1] The order then lists "without limiting the generality of the foregoing," specific topics, to wit: "(a) developments in the supply of raw materials and energy sources; (b) the growth to be expected in the population of Canada and the changes in its distribution; (c) prospects for growth and change in domestic and external markets for Canadian productions; (d) trends in productivity and standards of living; and (e) prospective requirements for industrial and social capital."[2]

In executing this assignment, the Commission journeyed extensively in Canada, held public hearings in 14 cities from October 1955 through March 1956, heard more than 750 witnesses, and received 330 submissions. It also sponsored the 33 monographs listed in the appendix, most of them prepared by the Commission's staff but some by other organizations and individuals. The present review is limited to the published studies and the *Final Report*;[3] and for the most part concentrates on the monographs.

The grouping of the monographs in the Appendix below, like any classification of a complex and somewhat heterogeneous product, is partly arbitrary. But the three broad groups distinguished—basic aggregates, industry studies, and foreign economic relations—suggest the main divisions of the inquiry and are a guide to the order followed in subsequent discussion. We deal first with population, labor force, product, and capital and their quantitative projection over the next 25 years. These constitute the framework for deriving the projected volumes of consumer expenditures and housing and social capital, and

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[1] See *Final Report*, p. 471, in the classified list of publications in the Appendix to this paper. Throughout the paper monographs are referred to by number as given in this list.

[2] *Ibid.*, pp. 471-72.

[3] A review of the *Preliminary Report* by D. McC. Wright appeared in the June 1958 issue of this journal, pp. 463-65.

for consideration of regional problems; and most of them are indispensable bases for evaluating prospects of specific industries and trends and problems in the area of international economic relations. Next, we touch upon the industry studies, but because there are so many and they are so specialized, our comments can do less justice to them than to the other groups of monographs. We then turn to the studies dealing with the international economic relations of Canada, particularly with the United States. A few general comments on the scope of the inquiry conclude the review.

I. *The Aggregates and Projections*

Monograph 2 is the key study of the aggregates and, in fact, of the whole range of inquiry of the Commission. Drawing partly upon the review of growth experience in monograph 1, partly upon the longer record back to 1926, partly upon some general knowledge of intersectoral differences in productivity and capital requirements, Hood and Scott follow a simple but systematic procedure. They derive first a projection of total population by age and sex—with three variants for differing amounts of *net* immigration; then a projection of the labor force, with a distinction between the armed services (kept by assumption at a constant level of 120,000) and the civilian labor force, the latter reduced to the employed by an allowance of 3 per cent for unemployment. Next the employed civilian labor force is distributed among three major sectors—agriculture, business, and civilian government and community services; and for each, gross domestic product per worker in constant prices is projected —with an allowance for a downward trend in average hours in agriculture and in business and with two variants of rates of growth of product per man-hour in the latter. The number employed is multiplied by projected product per employed in each sector and to the total, after some minor adjustments, residential rents are added to yield gross domestic product at factor costs. Next, indirect taxes are added, at a flat 13 per cent mark-up, to give gross domestic product at market prices; and a projection of net payments abroad of dividends and interest provides the final step by which gross national product at market prices is derived. These estimates are then used to project new nonresidential construction and durable equipment by an extensive use of gross capital-output ratios (combined with depreciation adjustments) for the various sectors; and, by reference to the sectoral estimates and to estimates in other monographs, the industrial distribution of gross domestic product is estimated.

Many of the estimates in monograph 2 provide the basis for projecting consumer expenditures in monograph 4. Personal expenditures on goods and services are derived from gross national product, in two steps: the first yields personal disposable income as a ratio to gross national product (expenditures) varying from 67 to 68 per cent; the second yields aggregate consumption by an allowance for personal savings of about 6 per cent of personal disposable income. Then total personal expenditures on goods and services, in constant prices, are reduced to a per capita basis, and on the basis of changes in the structure of consumer expenditures from 1926-29 to 1952-55 the total projected to 1980 is distributed among 9 major categories and a few subdivisions (4 in

household operation, 3 in transportation, and 3 in personal and medical care). In monograph 5, population estimates, combined with a projection of further urbanization and consideration of income trends, yield estimates of *needs* for new residential construction; and for each social capital category—hospitals, schools and universities, roads and streets, waterworks, sewerage systems, churches and other religious buildings—the relation of construction and capital needs to various specific indexes in the past is briefly discussed and a global projection is derived for 1956-80. Here also extensive reliance is placed on the projections in monograph 2, and on some of those in monograph 4. Monograph 6, on regional aspects, is the only study in which projections are limited to population and labor force; while the quantitative aspects of present interregional differentials and problems are pointed up by means of a variety of specific information, only a qualitative appraisal of growth prospects is given.

Two additional comments may clarify the nature of the quantitative framework of the inquiry. The first relates to the broad assumptions, explicitly stated, which govern the whole cast of the study—in the monographs and in the *Final Report*. There were four of these, and they are, as stated in the *Final Report*: (1) "that a global war will be avoided" (p. 3), although sizeable defense expenditures will continue; (2) "that although there will continue to be cyclical business fluctuations, the recurrence of a major depression such as that of the 1930's need not be anticipated" (pp. 3-4); (3) "that over the next twenty-five years there will be no changes either in the general price level or in price relationships" (p. 4), although this assumption is clearly recognized as unrealistic and elsewhere is stated somewhat less categorically (e. g., in monograph 2, p. 2, "it is assumed that the eroding force of inflation will be restrained"); (4) "that there will be no major changes in the economic policies either of the Canadian or other governments" (p. 4), again, as stated in the text, an assumption as unrealistic as assumption (3) but just as necessary if projections are to be made. Clearly, these assumptions impart a special meaning to the estimates, to be commented upon below.

Second, the forecasts require some specific procedures for projecting past levels or rates of change into the future—at numerous points in the sequence of steps only barely sketched out above. While it is impossible to describe these procedures in detail here, we can characterize them as essentially empirical judgments involving free-hand extrapolations of the past, not rigidly defined functions fitted mathematically to past records and then calculated for the future. To illustrate, in the projections in monograph 2, mortality rates for the separate age and sex groups are gradually reduced—for male infant mortality from 36 per thousand in 1954 to 16 in 1975-80 (and for females, from 28 to 13)—in view of the rates in other countries and of the continuous reductions made in recent decades; rates of labor force participation by age and sex are allowed to drift gradually up or down in accordance with recent trends; rates of growth in product per man or man-hour accord with recent changes in those rates in the major sectors of the civilian economy; and rates of change in the distribution of consumer expenditures by major categories also follow recent changes. The experience in the United States, with its sub-

stantially higher per capita income and relatively well-studied records, which may foreshadow the growth of Canada over the projected period, is also referred to frequently.

What do the projections show? A few figures, given in Table 1 with comparable estimates for the past 25 years, will perhaps suffice. Possibly, we should also note that the projected percentage distribution of gross national product (expenditures) among personal expenditures, government purchases, and capital formation will not differ much from the early 1950's; but with the rise in per capita personal expenditures, the shares of expenditures on food, clothing, and rent will continue to decline and the shares of expenditures on autos, electrical household appliances, and medical care will continue to rise.

One would expect the projected rates of growth to be higher than the rates for the 25 years covered by the quantitative records used so extensively in deriving the projections, viz., the late 1920's to the mid-1950's, since this particular period was affected by the great depression of the 1930's and the second world war. However, two aspects of these projections deserve note. First, the rates of growth that they suggest seem high in comparison with the rates for equally long periods in Canada's past back to the mid-19th century. The natural increase in the projected population estimates is 41 per cent (of the initial population) for 1955-75, and 40 per cent for 1960-80; while growth in projected total population, based on the middle assumption for immigration is 54 per cent for 1955-75 and 52 per cent for 1960-80 (monograph 2, Table 4.15, p. 175). Of the 9 overlapping periods of two decades each, covering the span of a century (1851-1951), in only 3 (1851-71, 1861-81, and 1901-21) is the rate of natural increase as high as or higher than those projected, and in only 2 (1851-71 and 1901-21) is the rate of increase of total population over 50 per cent (*ibid.*, Table 4, p. 156). Comparison with past rates of growth of gross national product per capita also suggests that the projection is high. According to O. J. Firestone, per capita gross national expenditure in constant (1935-39) dollars rose at the rate of 17.8 per cent per decade from 1867 to 1900; 16.1 per cent per decade from 1900 to 1929; and 19.3 per cent per decade from 1929 to 1953.[4] The projected rate of growth for 1955-80, using the average of the two variants, is 22.8 per cent per decade.

Second, whereas the over-all rate of growth for the projected 25 years is relatively high, the change in the structure of national product is moderate—at least when compared with the immediately preceding past. Thus from 1927-29 to 1953-55 the share of personal consumer expenditures in gross national product declined from 72 to 63 per cent while the share of government expenditures rose from 10 to 18 per cent; but the shares of these two major components in 1979-81 are set at 64 and 17 per cent respectively—not very different from the shares in 1953-55 (see monograph 2, Table 7.4, p. 318). A similar impression is conveyed by the distribution of gross domestic product (excluding residential rent and the gross product of the armed forces) among 7 major industry sectors. If we measure the total shift from 1927-29 to 1953-

[4] See his *Canada's Economic Development 1867-1953*, Income and Wealth, Series VII, International Association for Research in Income and Wealth. London: Bowes & Bowes, 1958. Table 10, p. 66. See pages 431-33 this journal for review of the book.

TABLE 1—BASIC AGGREGATES AND PERCENTAGE CHANGES, PAST AND PROJECTED 25-YEAR PERIODS[a]

	Past Period			Projected Period		
	Initial Absolute Figure (1)	Terminal Absolute Figure (2)	Per Cent Change (3)	Initial Absolute Figure (4)	Terminal Absolute Figure (5)	Per Cent Change (6)
1. Population (million)	10.46 (1930)	15.57 (1955)	+ 49	15.57 (1955)	26.65 (1980)	+ 71
2. Labor force (million)	4.06 (1930)	5.68 (1955)	+ 40	5.56 (1955)	9.93 (1980)	+ 79
3. GNP, 1949 prices, ($ billion)	8.45 (1926–29)	20.33 (1952–55)	+133	21.6 (1955)	61.75 (1980)	+186
4. GNP, per capita, 1949 prices ($)	863 (1926–29)	1,355 (1952–55)	+ 54	1,387 (1955)	2,317 (1980)	+ 67
5. Personal expenditures, per capita, 1949 prices ($)	597 (1926–29)	880 (1952–55)	+ 45	880 (1952–55)	1,560 (1980)	+ 72
6. Gross domestic investment, 1949 prices (col. 1–3) and 1955 prices (col. 4–6) ($ billion)	2.26 (1927–29)	4.99 (1953–55)	+114	6.3 (1953–55)	18.3 (1979–81)	+179

[a] Years to which estimates refer are shown in parentheses. Whenever the aggregates refer to a period longer than a year, the average per year is given. The percentage change in columns 3 and 6 is reduced to a 25-year basis whenever the period covered is longer.

Sources:

Line 1: from monograph 2, Tables 4.14 and 4.15, p. 175. The 1930 figure is adjusted upwards by 2.6 per cent to allow for the inclusion of Newfoundland; and the projected figure is that based on the assumption of 75,000 annual immigration.

Line 2: columns 1 and 2 from monograph 29, Table 35, p. 226, and include armed forces; columns 4 and 5 from monograph 2, Table 4.23, p. 186, and exclude armed forces.

Line 3: columns 1 and 2 from monograph 29, Table 36, p. 228; columns 4 and 5 from monograph 2, Table 5.20, p. 226. The mean of the high and low estimates (based on two variants of the growth in product per worker in the business sector) is shown here.

Line 4: columns 1 and 2 from monograph 29, Table 36, p. 228; columns 4 and 5 derived from lines 1 and 3 above.

Line 5: from monograph 4, Table 1, p. 3.

Line 6: columns 1 and 2 from monograph 2, Table 7.D.2, p. 506. This table shows domestic investment excluding government nondefense investment expenditure. The latter was added on the basis of its percentage shares in the totals in current prices for 1927-29 and 1953-55 (11 per cent and 17 per cent respectively), as shown in *ibid.*, Table 7.6, p. 324. Columns 4 and 5 from *ibid.*, Table 7.6, p. 324. The average in column 4 is in current prices but would differ little from the figure in 1955 prices.

55 by taking the differences between the percentage shares for the two dates for each sector and adding them regardless of sign, the resulting index of change over the past 26 years is 25; the same index for the projected changes from 1953-55 to 1979-81 is only 19.8 (*ibid.*, Table 7.2, p. 315). To be sure, the major reason for this moderate change is that the share of agriculture, already low in 1953-55, could not be reduced absolutely as much as its higher share in 1927-29; and the results might be different if a more detailed classification of the nonagricultural division were available. But the more detailed distribution of consumer expenditures among 15 categories yields a similar result: from 1926-29 to 1952-55 the over-all measure of shift is 19 whereas from 1952-55 to 1980, a slightly longer period, it is 13.3, almost a third lower (monograph 4, Table 26, pp. 74-75).

In evaluating the projections, it is helpful to distinguish between questions relating to: (a) the general meaning of the forecasts; and (b) the degree of confidence that can be attached to the quantitative results, given the empirical data and the methods used in the estimation.

(a) Projections of the type involved in this inquiry can be characterized as measures of the probable course of an economy—given the major assumptions or conditions actually used in preparing them. The central question is then one of defining the assumptions—as they were *used,* not as stated. The reason for the distinction becomes clear if we ask: Were *all* four major assumptions, explicitly stated and quoted above, indispensable conditions upon which the projections were calculated, and were they the only major ones?

There is no doubt that an all-out war and a major depression like that of the 1930's were excluded from consideration in preparing the projections. The high rates of over-all growth and the relatively moderate structural changes projected are, in themselves, evidence of this exclusion. But is it evident that continuing price rises of the magnitude that occurred during the years following the second world war were excluded? To be sure, the projections are given in constant dollars, and neither the probable changes in the general price level, nor, with few exceptions, the differential price movement for the several sectors in the economy are estimated. But this does not mean that the projection is explicitly *conditioned* by an absence of general or differential price changes—in the sense that any future price changes, unless they represent a "runaway" inflation (whatever this somewhat vague term means) would seriously invalidate the projections, as an all-out atomic war or a great depression would. On the contrary, one might argue that continuation of sustained price rises, of a magnitude whose limit would have to be explored, would make the calculated projections more rather than less probable. Since they have been derived from a record of the past in which prices did increase substantially—the price index implicit in gross national expenditures having risen from 77 in 1945 to 124 in 1955 (1949 = 100), or over 60 per cent (see monograph 4, Table 4, p. 33)—the projected high rates of growth combined with relatively full employment are perhaps more likely to be realized with rising rather than constant prices. In short, the stated assumption of price constancy is merely an indication of the omission of changes in prices from the projection, not a *condition* of its validity.

A similar question can be raised about the assumption relating to policy changes. What exactly is meant by absence of "major" policy changes on the part of the Canadian and *other* governments? To begin with, the most revolutionary changes in a number of the 100-odd sovereign states in the world are unlikely to have much effect on the economic progress either of Canada or of the countries important to it. But even if we consider only Canada and its close neighbors and partners in the network of international economic relations, the vague term "major" leaves room for misconception. Canada experienced rapid growth during the recent decades when, if we can judge by contemporary reactions, substantial changes in government policy did occur, not only in Canada but also in the United States and other close partners (e.g., a phenomenal increase in the personal income tax burden, an almost revolutionary change in foreign aid policy, etc.). Could it not be argued that the projection would stand so long as changes in government policy do not inhibit economic growth any more than they have in the past? With the historical period in the past specified, the assumption underlying the conditional nature of the projection could then be defined properly.

Thus, as the above comments suggest, of the four assumptions stated one is superfluous and one is too vague; and, moreover, a host of governing assumptions have not even been mentioned. Some are so obvious that they need no explicit statement, e.g., the prospective stability of the universe in which mankind, Canada included, lives, a stability easily assumed over periods that are long in human terms but short for universal natural processes. Others, however, that are readily accepted, are not too obvious. Clearly, in making *any* projection, we are assuming the continuation of the cumulative effect of human knowledge on economic production; and in particular the continuation of the dynamism of a country's economic and social institutions, of the search for greater economic product by its inhabitants, and of the country's capacity to adjust its institutions to changing conditions. This complex of what might be called constitutive assumptions (as distinct from the conditional ones, exemplified by exclusion of a war or a major depression) is at the base of every projection; and unfortunately these basic assumptions cannot be tested by any meaningful probability approach—except perhaps an impossibly wide study of many large human societies existing under differing conditions. Since these assumptions underlie the whole of the present inquiry, we can reformulate our definition of the projections and describe them not as *forecasts* but as the quantitative *implications* of the assumed continued dynamism of the Canadian economy and society, on conditions of absence of an all-out war and a great depression.

(b) If the projections are quantitative implications of an assumed set of social institutions and drives, their reliability can be appraised only by considering the empirical record on which the projection coefficients have been based. The crucial questions are then: how stable were the coefficients in the past under a variety of relevant changes covered in the record (relevant meaning those that could affect growth); and how effective were the procedures used to establish these coefficients? Thus the most reliable basis for projections requires records for many long periods and for many countries, the

periods excluding major wars and great depressions and the countries belonging to a group of which Canada could be considered a unit with some unique but also many representative features. Analysis of these records would reveal the patterns of growth over spans of 25 years that proved stable under a variety of relevant changes. These common and stable features would constitute empirical generalizations, whose relation to each other could be tested by economic and statistical theory. This basis for projections would, of course, have to be modified for any one country to take account of features unique to it. In our present state of ignorance of quantitative patterns of the comparative economic growth of nations, no such basis for projection is available, or likely to become available for years to come.

Consequently, the judgments necessary for a quantitative projection can be checked only by a narrow body of organized empirical data. In the present inquiry this body was limited almost exclusively to the record for Canada since 1926, with repeated reference to recent data for the United States and to the absence of detailed and consistent data for Canada (except for population) for years before 1926. But some measures of the economic growth of Canada for longer periods do exist—although less detailed and subject to wider errors than the data for the last 25 years—and at least one recent attempt to organize and interpret them has been made (by Firestone; see footnote 4). It is not clear why no effort was made to utilize the longer records and why a projection over a 25-year period was built essentially on the empirical record for one period of comparable length in the past. The time span from 1926 to 1955 is altogether too short to provide an objective basis for projections, especially since over half of it was affected by the great depression and the second world war. It is, of course, this circumstance that necessarily minimizes the use of mathematically fitted trend lines and compels extensive exercise of judgment and of free-hand interpolation and extrapolation.

It may also explain the rather high over-all rate of growth and the rather moderate extent of structural changes in the projections. With the empirical record limited to 1926-55, even a line connecting the late 1920's with the early 1950's may underestimate the probable rate of growth, since it covers the years of the great depression. The least affected span within the total period is the decade following the second world war, but even then adjustment must be made for the transitory effects of the readjustment immediately following the end of the war. The rates of growth in that decade were quite high: population grew 26 per cent from 1945 to 1955 (adding 2.6 per cent to the earlier year for Newfoundland) and gross national product per capita (in 1949 prices) grew at a decade rate of 20 per cent from 1948-50 (to omit the readjustment years after the war) to 1953-55. The projected rate of growth of population, using the middle assumption for immigration, is 24 per cent per decade; and that for gross national product per capita, using the mean of high and low, is 23 per cent per decade. It is tempting but unfair to say that the projected rates are merely extrapolations of rates observed in the short postwar period. However, it may be fair to suggest that, given the rapid growth in the recent decade and no explicit reference to the longer-term past in Can-

ada and elsewhere, it would have been difficult to defend appreciably lower rates of growth in the projection, or consider much higher ones.

But what of the narrow range of structural change suggested by the projections? This result may seem plausible since the depression and major war might have produced wide structural changes in the past 25 years, even though total population and per capita product did not grow as rapidly as it is assumed they will grow in 1955-80. But ordinarily a high rate of growth, particularly of per capita product, is associated with large changes in structure, in the distribution of product by both industrial source and type of use, at least within consumer expenditures. One wonders whether the brevity of the empirical record of reference, combined with some of its peculiarities, has not led to an underestimate of the structural changes projected. To illustrate, with per capita consumer expenditures rising from 1952-55 to 1980 by 77 per cent, the part spent on food drops from 27 to only 24.5 per cent, thus allowing for a rise in per capita expenditures on food of 60 per cent—whereas from 1926-29 to 1952-55, with per capita expenditures rising only 47 per cent, the part spent on food dropped from 30 to 27 per cent. Why should there be a similarly small decline in the share of expenditures on clothing and personal furnishings —from 12.4 to 11.2 per cent, whereas the decline over the past 25 years was from 13.9 to 12.4 per cent? To be sure, past changes in shares should not be extrapolated without regard to limits set by arithmetic and by the gradual exhaustion of the change-potential; and all long-term projections may tend to underestimate structural shifts because of the difficulty of visualizing changes that can radically modify the current structure, which is, after all, the springboard for the leap into the future. It is perhaps because of lack of knowledge of what new goods will emerge in the future to displace those which, like food and clothing, have low secular income elasticity of demand, that the shares of the latter in consumer expenditures were not reduced further—for such reductions would only swell unrealistically the shares of the more rapidly growing goods or of the quite meaningless "miscellaneous" group into which the unknown new goods would have to be thrown. Yet the question remains whether a longer historical perspective would not have led to a somewhat bolder treatment of changes in structure.

In the light of these comments one may conclude that the margin of error in the projections is large, certainly much larger than those shown in the summary of forecasts of gross national product in monograph 2 (p. 224). The ±7.7 per cent shown for gross national product (for the estimate of population based on 75,000 annual net immigration) is merely the range between the low and high estimate of the rate of growth in product per worker in the business sector. Clearly, compared with the middle projection of growth in gross national product per capita of 23 per cent per decade, the rate could easily be as low as 15 or as high as 30 per cent (it averaged 29 per cent in Sweden over a long period).[5] And correspondingly, the structural changes

[5] Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations: I. Levels and Variability of Rates of Growth," *Econ. Develop. and Cult. Change*, *5* (1), Oct. 1956. Table 1, p. 10.

could be wider or narrower than those suggested—in positive association with the rate of growth of the aggregates, particularly of product per capita. For reasons suggested above, defensible limits of the range cannot easily be set. Yet it is regrettable that no such attempt was made in the inquiry.

II. *The Industry Studies*

The list includes eighteen monographs in this group, and it could be extended to cover monographs 30 and 32. These reports range widely in size—from pamphlets of less than 50 pages to weighty tomes of more than 400 pages; and in scope—from those dealing with major complexes of industries accounting for substantial shares of product and labor force, e.g., agriculture, mining and mineral processing, energy, secondary manufacturing, and service industries, to those concerned with limited local problems, e.g., the Nova Scotia coal industry. There are also notable differences in treatment, resulting partly from the large number of authors, some individual and some institutional, and partly from differences in availability of basic data and in the character of the problems in the respective sectors. It is impossible to provide anything approaching an adequate review here. One can only note the general pattern of the studies, some of the major conclusions, and a few questions that they raise.

In all the studies the first task was to provide a consistent and fairly detailed quantitative description of the specific industrial sector—its output, the labor force engaged in it, the variety of its products, the degree of its reliance upon domestic and foreign markets, the extent of competitive pressures on it from abroad. And for all, a historical, quantitative record usually extending back only to the late 1920's and sometimes for a shorter period is provided, although illuminating comments on the earlier decades are made for the sectors that played important parts in Canada's history. The concluding, and much briefer, parts of the monographs deal with the projection of future trends, usually linked with those for population, total product, and expenditures per capita; and the problems suggested by such projections are often raised, and sometimes discussed at length. For some sectors, e.g., energy, forest industries, and minerals and mineral processing, the question is largely whether the expected growth in demand—domestic and foreign—can be adequately met out of the resources, usually natural, available in Canada. For others, e.g., agriculture, fisheries, and secondary manufacturing, the emphasis is upon the possible limits to demand—either because of limited domestic markets or because of competitive pressures from abroad (often the United States); and the problem is one of adjustment of production and employment to these possible limits upon demand. And, of course, in the monographs on major sectors, which comprise a range of industries differing in their economic characteristics and growth potentials, there is detailed consideration of these components and the past and prospective shifts in weight among them.

Without question, these 18 monographs constitute a most valuable reference library on the past development, current structure, and prospective trends of Canadian industry. It is difficult to think of a collection of similarly competent, systematic, up-to-date industry studies for any other country. Despite the

uneven quality of the analysis and writing—inevitable with so many authors and with studies completed in the relatively short period of about two years—the wealth of data organized, the skill of the presentation of the major trends and problems, and the generally high level of interest evoked are all most impressive, at least to this reviewer who is not too familiar with industrial economics in general, or with Canada's industries in particular. The monographs by John Davis on mining and mineral processing, and on energy prospects; by Drummond and Mackenzie on agriculture; by Fullerton and Hampson on the secondary manufacturing industries; by the Royal Bank of Canada on the construction industry; by J-C. Lessard on transportation (containing some valuable calculations of costs and burdens of competing forms of transportation); and by the Bank of Montreal on the service industries are especially to be noted.

What do these monographs show with respect to differences in growth prospects for the various industries? In considering these differences, and the shifts in the industrial structure of Canada's national product and labor force over the next 25 years, we can perhaps most effectively summarize the contribution of these monographs in Table 2, which indicates the broad conclusions. The coverage of the specific industrial sectors is given in the notes to the table.

Some of the shifts in the structure of gross domestic product and of the civilian employed labor force, both past and projected, accord with what we would expect in a developed country in the process of further growth. Thus the decline in the share of agriculture in both product and labor force is marked over the last 26 years and is projected to continue further to 1980. Likewise, the rise in the share of total manufacturing in gross product is large from 1927-29 to 1953-55 and is projected at an appreciably lower rate of rise to 1980; and the rise in its share in the labor force over the past 26 years is to be succeeded by a slight drop from 1953-55 to 1979-81. Finally, the rise of the shares of the service groups in the labor force, particularly trade and finance, government, and all services, is accompanied by a failure of their shares in gross domestic product to rise. All of these trends have been observed in other countries, e.g., the United States.[6]

Yet two Canadian trends appear to be distinctive. One is the large rise in the share of the resource industries in gross domestic product, although not in their share in the labor force—a reflection of Canada's growing importance as a major supplier of industrial raw materials, associated with recent discoveries and with the shift of its neighbor, the United States, to the position of a raw material importer. While the discoveries may seem to be accidental, it is legitimate to argue that with the growth of Canada's population density, with the shift of its population to the West, and with the expanding market for raw materials, the discoveries are largely a function of more intensive exploration and demand rather than accident. The other distinctive element is that revealed by the differentiation between primary and secondary manufacturing—

[6] For a discussion of the trends in the shares of industrial sectors in income and labor force see Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations: II. Industrial Distribution of National Product and Labor Force," *Econ. Develop. and Cult. Change*, Suppl. *5* (4), July 1957.

TABLE 2—STRUCTURE OF GROSS DOMESTIC PRODUCT (EXCLUSIVE OF RESIDENTIAL RENTS AND GDP ARISING IN THE ARMED SERVICES SECTOR) IN 1949 DOLLARS AND OF THE CIVILIAN EMPLOYED LABOR FORCE, AND RELATIVE INCOME PER WORKER, PAST AND PROJECTED

	Percentage Change		Percentage Shares or Relatives		
	From 1927–29 to 1953–55 (1)	From 1953–55 to 1979–81 (2)	1927–29 (3)	1953–55 (4)	1979–81 (5)
A. Structure of Gross Domestic Product[a]					
1. Agriculture	10	31	23.4	12.7	5.7
2. Resource industries[c]	198	379	6.4	9.4	15.4
3. Primary manufacturing[d]	178	196	5.3	7.2	7.2
4. Secondary manufacturing[e]	173	232	16.5	22.3	25.3
5. Total manufacturing (3+4)	174	224	21.8	29.5	32.5
6. Construction[f]	124	159	5.7	6.3	5.6
7. Transportation, storage, and communication	89	n.a.	8.9	8.3	n.a.
8. Trade, finance, and services[g]	95	n.a.	24.8	23.6	n.a.
9. Total transportation and trade (7+8)	93	200	33.7	31.9	32.8
10. Civilian government and community services[h]	127	131	9.0	10.2	8.0
11. Total	103	192	100.0	100.0	100.0
B. Structure of Civilian Employed Labor Force[a]					
1. Agriculture	−30	−13	33.1	16.2	7.6
2. Resource industries[c]	35	103	5.5	5.2	5.8
3. Primary manufacturing[d]	62	60	5.1	5.8	5.0
4. Secondary manufacturing[e]	90	82	15.0	20.0	19.8
5. Total manufacturing (3+4)	83	77	20.1	25.8	24.8
6. Construction[f]	101	78	4.8	6.7	6.5
7. Transportation, storage, and communication	35	n.a.	8.2	7.7	n.a.
8. Trade, finance, and services[g]	80	n.a.	21.1	26.5	n.a.
9. Total transportation and trade (7+8)	67	116	29.3	34.2	40.4
10. Civilian government and community services[h]	140	128	7.2	12.0	14.9
11. Total	43	83	100.0	100.0	100.0
C. *Gross Domestic Product per Employed Worker*[b]					
1. Agriculture	58	52	0.71	0.79	0.75
2. Resource industries[c]	121	135	1.17	1.81	2.67
3. Primary manufacturing[d]	72	85	1.03	1.24	1.44
4. Secondary manufacturing[e]	44	83	1.10	1.11	1.28
5. Total manufacturing (3+4)	50	83	1.09	1.14	1.31
6. Construction[f]	12	46	1.20	0.94	0.86
7. Transportation, storage, and communication	41	n.a.	1.08	1.07	n.a.
8. Trade, finance, and services[g]	8	n.a.	1.17	0.89	n.a.
9. Total transportation and trade (7+8)	16	39	1.15	0.93	0.81
10. Civilian government and community services[h]	− 5	1	1.27	0.85	0.54
11. Total	42	59	1.00	1.00	1.00

(Footnotes at bottom of page 371)

a valuable distinction not ordinarily made in the literature. While the shares of both in national product grew from 1927-29 to 1953-55, only the share of secondary manufacturing is expected to continue to grow to 1979-81—implying a shift within manufacturing in favor of the secondary industries with a similar but more limited shift within the labor force employed in manufacturing.

While the summary of the past record and the projections in Panels A and B of Table 2 suggest plausible trends in the structure of gross domestic product and the employed labor force, those in gross domestic product per worker in Panel C raise some questions. In some sectors, the projected rise in product per worker is not too different from that found over the past 26 years: e.g., in agriculture, with its 50 to 60 per cent rise both in the past and in the projected span to 1980; in the resource industries, with their more than doubling of product per worker, both from 1927-29 to 1953-55 and from the latter date to 1980; and in primary manufacturing, with its rise in product per worker by about three-quarters over the last 26 years and more in the span to 1980. In all these cases, the past 2½ decades have witnessed impressive rises in product per worker despite the depression and the war, and perhaps partly because of them; and the projection of these or only slightly higher rates to 1980 does not raise obvious questions.

But in some sectors—secondary manufacturing, construction, and the combined group of transportation and trade—the projections involve a marked acceleration of the rate of growth of product per worker. It may help to indicate the basis for these projections. For secondary manufacturing the projections anticipate an "annual rise in output per man-hour of close to 3¼%" (monograph 16, p. 183), and the productivity assumptions have "been based on three sources, roughly in order of importance: (a) The performance of each industry since the war with particular importance attached to its record since 1949; (b) Guesses about future productivity in the studies and submissions; (c) The long-term trends, 1926-1955" (*ibid.*, p. 186). For construction "the

[a] Panels A and B from monograph 2, Tables 7.2 and 7.1, pp. 315 and 311.

[b] Panel C derived by dividing entries in Table 7.2 by those in Table 7.1 and calculating the appropriate percentage changes and sector relatives to the countrywide gross domestic product per employed worker.

[c] Resource industries include forestry, fishing, mining, quarrying and oil wells, and central electric stations.

[d] Primary manufacturing includes industries involving relatively minor processing of domestic resources—canning and processing, dairy products, grain mill products, meat products, saw and planing mill products, pulp and paper, nonferrous metal smelting and refining, abrasives, cement, and primary chemicals.

[e] Secondary manufacturing includes all other manufacturing industries involving more elaborate processing and often using imported raw materials or parts.

[f] Construction includes contract construction only; excludes own-account work done by private units and government departments not primarily engaged in the construction industry.

[g] Trade, finance, and services includes wholesale and retail trade; finance, insurance, and real estate; and business, personal, and recreation services.

[h] Civilian government and community services includes, in addition to government, educational, health, religious, welfare, and other community services.

nature of the industry will prevent any dramatic increase in productivity" but in view of preceding discussion which "outlined many reasons for believing that the industry will become increasingly dynamic and progressive . . . an increase of approximately 2% per annum in construction productivity is probably the best guess that can be made for the quarter century between now and 1980" (monograph 22, p. 214). The authors then add that "this is well above the long-term average of the past, although below the higher rates achieved in the special circumstances of the last few years" (*ibid.*, p. 214). Clearly, in these two major sectors the basis for projecting higher rates of growth in product per worker is largely the experience of the few years after the second world war; and the comments in the preceding section, concerning the tenuousness of extrapolating from so short a period, apply here.

But in the trade, finance, government, and service sectors results are even more puzzling. If we set the share in the labor force of the transportation, storage, and communication sector at 7.7 per cent in 1979-81 (i.e., at its 1953-55 level in the table), and assume that its product per worker at that date would equal 1.07 (the level for 1953-55), its share in gross domestic product in 1980 would be 8.3 per cent. This would leave a share in gross domestic product for trade, finance, and service in 1980 of 24.5 per cent, a share in the civilian employed labor force of 32.7 per cent, and a product per worker relative to the countrywide product per worker of 0.75. Yet the implied rise in product per worker from 1953-55 to 1979-81 would be 34 per cent, compared with an 8 per cent rise from 1927-29 to 1953-55.

To be sure, the shares of some service sectors in the table, particularly those for the combined group of transportation and trade (line 9), are residuals derived from the total projection for business by subtracting those for the other specific industrial divisions. But disregarding for present purposes the exact nature of these projected shares, one may properly raise two sets of questions. First, what meaning can be attached to constant price estimates of total product, and hence of product per worker, in the whole range of service industries? This question is sharpened by the findings that the product per employed worker in government and community services declined and that in trade, finance, and other services barely rose from 1927-29 to 1953-55. To what extent have these results been due to shifts within each sector (presumably from high product per worker to low product per worker segments)? To what extent have they been due to the fact that, with general price rises over the period, the groups in question did not possess the bargaining power to secure rises in their income that would have at least allowed for whatever increase in real product did in fact occur? To what extent has the lag in property incomes, particularly those with fixed or sluggishly changing returns, reduced total product and product per worker in the trade and finance sector? If these questions are relevant, the measures of product are much affected either by large internal shifts or by changes in bargaining power under conditions of sustained and large price rises and full employment, or by both.

Given these, the second set of questions follows. If the movement of gross domestic product per worker from 1927-29 to 1953-55 in the service sectors, and perhaps also in trade and finance, has been due in substantial part to

internal shifts and/or inflation and resulting changes in the bargaining power of various income recipient groups, should the projection extrapolate this trend into the future—as was done for the government and community services sector, if not for trade, finance, and other services? Can one assume the same internal shifts? Can one assume that at a constant per worker product—which presumably means little rise in per worker real income—there will be a flow of labor into the government and community services sector which will raise the share, as projected, from 12 to 15 per cent of the total employed labor force? How is this projection reconciled with the increased weight of educational and health services in the government and community services sector and with the need for and advocacy of higher rates of return in education and health to attract the necessary human resources? Is it reasonable to expect that trends will be allowed to continue which would by 1980 bring per worker product, as a relative of countrywide per worker product, as low as 0.54 in government and community services, and as low as 0.75 in trade, finance, and other services? This last implication could be checked only if we had a distribution of total product for each sector between that flowing to the workers engaged in it and that flowing elsewhere; and it is regrettable that no such distributions are available in the monographs. But it seems clear that the implied movement of product per worker in the trade, finance, and service sectors raises puzzling questions that cast some doubt upon the validity of the projections.

With respect to this large segment of the country's economy, the inquiry under review, like almost all similar studies for developed economies, faces difficult problems of measurement. Such problems are serious enough for the commodity-producing and transporting segments of the economy; but for them we have quantity volumes that, for all the qualifications involved in measuring quality and the effects of changing weights, do permit some acceptable approximation to volumes in constant prices. No such bases are available for the services segment of the economy, unless the compensation of factors is treated as a measure of output on the possibly unrealistic assumption that the markets in themselves assure its consonance with marginal productivity and hence with output; or unless the volume of services is derived as a direct function of the volume of commodities handled by the economy. In the former case, the results are likely to reflect internal shifts and the bargaining weakness of the factors involved under conditions of price rises and full employment; in the latter case, the share of services in national product remains relatively constant in the long run, and independent measurement of the product of a sector that accounts for an increasing share of the total labor force is sacrificed.

These comments are made not because another answer or treatment can easily be suggested, but rather to stress the problem and the need for a more detailed and incisive approach, which would at least try to probe the difficulties and experiment with some alternative solutions. As matters stand, the whole cast of the inquiry reflects the emphasis put on the commodity-producing and transporting sectors of the economy and the relative neglect of the others. Of the 18 monographs in the list, just one, monograph 24, deals with the service industries—although it is full of valuable information and analysis.

Yet by 1953-55 at least a third of gross domestic product, and close to four-tenths of the labor force, are accounted for by the trade and service sectors; and in the projections for 1980 these shares are a third of product and close to half of the labor force.

III. *Foreign Trade and Capital*

With its relatively small population, abundant natural resources, and emphasis on individual business enterprise, Canada favors the kind of productive specialization that calls for extensive foreign trade as a complement and provides opportunities for foreign capital investment. Indeed, with the ratios of imports and exports to gross national product respectively about a quarter, and with long-term foreign capital amounting in 1955 to almost half of gross national product (and, assuming a capital-output ratio of 3 to 1, presumably a sixth of its total reproducible capital), Canada is most intensively engaged in the network of international trade and capital investment.

Table 3 presents the more important data and projections provided in monographs 25, 26, and 29 by Slater, Anderson, Brecher, and Reisman. The historical perspective here, at least for the over-all aggregates, is somewhat longer than in the other monographs, extending back to the beginning of the century; and the major trends are clearly discernible. The findings can be briefly summarized in three statements.

First, the projected trends, in continuation of those observed in the past, are toward a smaller relative weight of imports and exports in gross national product, and toward a lesser contribution of foreign capital investment. Within foreign trade in commodities, the shift in imports is away from food, clothing, and even some consumers' durables toward machinery and equipment; and the shift in exports is away from agricultural, animal, and forest products toward petroleum, minerals, and metals.

Second, the United Kingdom accounted for declining shares and the United States for rising shares in Canada's imports and exports, and in foreign capital invested in Canada. The growing share of the United States in foreign investment in Canada was accompanied by a shift from portfolio to direct investment: in 1926 portfolio investment was still 66 per cent of the total and direct investment only 30; by 1955, the share of the former declined to 38 per cent and that of the latter rose to 57 per cent (monograph 29, Table 18, p. 91). There was a related shift from debt to equity: the share of the former declined from 57 per cent in 1926 to 30 in 1955, whereas that of the latter rose from 43 to 70 per cent (*ibid.*, Table 19, p. 92).

Third, the projections carry forward the trends just observed in the shares of the United Kingdom and the United States, although at diminished rates. By 1980, the United States would, according to the projections, account for three-quarters of Canada's imports, seven-tenths of its exports, and well over eight-tenths of total foreign investment in Canada. There would presumably also be associated further shifts from portfolio to direct investment and from debt to equity.

Monograph 29 by Brecher and Reisman is devoted to the special problems of economic relations between Canada and the United States. One topic cov-

ered is the role of this country as foreign investor in Canada, some aspects of which have already been indicated, and it is further amplified by data on ownership and degree of control (the latter for United States direct investment corporations in Canada, with majority ownership by stockholders in the United States, including a few known to be controlled by parent firms in the United States, although with less than 50 per cent ownership of stock by nonresidents). For the branches of the Canadian economy with large amounts of foreign capital (manufacturing, petroleum, mining and smelting, railroads, other public utilities, merchandising, and construction), the share owned by all nonresidents declined from 37 per cent in 1926 to 32 per cent in 1954, but the share owned by United States residents rose from 19 to 25 per cent. The share *controlled* by nonresidents rose from 17 per cent in 1926 to 28 per cent in 1954, and almost all of the rise is accounted for by the United States, whose share rose from 15 to 25 per cent (monograph 29, Tables 24 and 25, pp. 100-101; the shares are of book value of capital invested).

The ratios for more narrowly defined branches are more significant. By 1955 nonresidents controlled 54 per cent of investment in Canadian manufacturing, of which United States controlled 45 per cent, compared with 35 and 30 per cent respectively in 1926. By 1955, nonresidents controlled 59 per cent of investment in mining, smelting, and petroleum exploration and development, of which the United States controlled 57 per cent, compared with 38 and 32 per cent respectively in 1926. Finally, "In a number of important Canadian industries—including oil and gas, nickel, aluminum, asbestos, iron ore, automobile, electrical, rubber and chemicals—a few enterprises owned and controlled by United States residents account for a preponderant share of total investment, output and employment in these industries" (*ibid.*, p. 155).

In addition to capital investment, ownership, and control, the monograph discusses other aspects of economic relations between Canada and the United States: the sensitivity of Canada's economy to business cycles in this country; the effects of United States tariff policy and of its agricultural policy; the trade union links between Canada and the United States; and concludes with a comparison of the economic growth of the two countries. The findings are not unexpected: the Canadian economy is sensitive to cyclical movements in the United States; the tariff and agricultural policies of this country have important repercussions in Canada, and while the trend toward lower tariffs and easier administration of customs is helpful, the "over-all impact of United States agricultural policy has likely been to restrict total farm output in Canada and to distort the pattern of Canadian agricultural production and exports" (*ibid.*, p. 194); "the trade-union movement in the United States has exerted a powerful and continuous influence on Canadian organized labor [but] Canadian autonomy is particularly strong in the key areas of collective bargaining, including the use of the strike weapon" (*ibid.*, p. 220); for the last 30 years (1926 to 1955) Canada grew at a somewhat higher rate than the United States and, if we compare projections (for the United States essentially that of the Paley Commission raised by 10 per cent to allow for an upward revision of population forecasts), Canada's prospective growth is likely to

TABLE 3—TRENDS IN AND PROJECTIONS OF FOREIGN TRADE AND INVESTMENT[a]

	Past			Projection
	Beginning of Century (1)	1920's (2)	1950's (3)	1980 (4)
Ratio of Foreign Trade and Investment to G.N.P. (percentages)				
1. Total imports	31.2 (1901–15)	29.0 (1921–29)	23.9 (1952–56)	19.7
2. Total exports	21.3 (1901–15)	28.6 (1921–29)	21.7 (1952–56)	18.4
3. Net foreign investment	9.9 (1901–15)	0.4 (1921–29)	2.2 (1952–56)	1.3
Percentage Share of Commodities				
4. In total imports		65.6 (1927–29)	71.3 (1953–55)	69.8
5. In total exports (inc. gold)		77.2 (1928)	76.8 (1955)	76.8
Percentage Shares of Selected Groups in Merchandise Imports (1955 in col. 3 throughout)				
6. Food, tobacco, and alcoholic beverages			12.0	10.2
7. Clothing, textiles, leather			12.1	10.8
8. Furniture, appliances, and misc. manufactured consumer goods			11.0	13.8
9. Machinery and equipment			20.2	27.7
10. Petroleum			8.2	5.3
11. Chemicals			4.1	6.7
12. Misc. industrial materials and cap. goods			14.6	14.4
Percentage Shares of Selected Groups in Merchandise Exports (1955 in col. 3 throughout)				
13. Wheat and wheat flour			9.5	4.3

[a] Years to which estimates refer are shown in parentheses.

Sources:

Lines 1-3: columns 1-3 from monograph 25, Table 19, p. 70. The absolute amounts were added for the periods, and the percentages calculated from the totals. Column 4 from *ibid.*, Table 59, p. 164.

Line 4: columns 2 and 3 from monograph 25, Table 3, p. 27; column 4 from *ibid.*, Table 58, p. 161.

Line 5: columns 2 and 3 from monograph 26, Appendix A, p. 299; column 4 from *ibid.*, Table 4, p. 119.

Lines 6-12: monograph 25, Table 32, p. 97.

Lines 13-21: monograph 26, Table 3, pp. 105 ff.

Table 3—(*Continued*)

14. Other agricultural and animal products			12.2	6.9
15. Lumber			8.8	5.7
16. Newsprint			15.3	11.8
17. Woodpulp			6.8	6.2
18. Petroleum and products			0.9	13.7
19. Aluminum and products			4.9	9.5
20. Copper products and nickel			9.0	6.2
21. Chemicals (ex. uranium)			4.3	5.7
Percentage Shares of U.K. and U. S. in Foreign Trade and Investment				
Shares in Merchandise Imports				
22. U.K.	24.0 (1900, 1912)	15.0 (1929)	9.5 (1953–55)	
23. U. S. (same years as line 22)	61.3	68.8	73.0	
Shares in Total Imports				
24. U.K.			9.4 (1955)	10.6
25. U. S.			73.4 (1955)	76.2
Shares in Merchandise Exports				
26. U.K.		22.2 (1926–29)	17.1 (1952–55)	
27. U. S. (same years as line 26)		38.3	57.9	
Shares in Total Exports				
28. U.K.			17.2 (1955)	14.2
29. U. S.			62.1 (1955)	69.5
Shares in Total Foreign Capital Investment				
30. U.K.	85 (1900)	44 (1926)	17 (1955)	
31. U. S.	14 (1900)	53 (1926)	77(1955)	

Lines 22-23: monograph 25, Table 6, p. 29. The entries in column 1 are arithmetic means of the ratios for fiscal 1900 and fiscal 1912; in column 3 of the ratios for each calendar year included.

Lines 24-25: monograph 25, Table 60, p. 164.

Lines 26-27: monograph 26, Table III, pp. 314-18. The absolute amounts were added for the periods, and the percentages calculated from the totals.

Lines 28-29: see source for lines 24-25.

Lines 30-31: monograph 29, Table 16, p. 88.

continue at a somewhat higher rate, further reducing the gap in per capita and per worker income between the two countries. Expected as the findings are, it is of value to have a systematic, and at many points detailed and quantitatively tested, treatment of all these aspects of economic relations between Canada and the United States—a treatment that assembles new data on several topics and breaks new ground.

In much of the discussion of Canada-United States relations, particularly in the *Final Report*, there is a conflict between apprehension over the recently increased "dominance" of the United States in the foreign trade of Canada, its foreign capital, and some of its important industries, and recognition that these ties with its larger neighbor to the south are an important and increasing source of economic strength, a basis for past growth and a promise for the future which it would be irrational to forego. The discussion recognizes that the shift in the character and identity of Canada's main creditors and trading partners is in large part a result of the country's greater maturity, attained with the completion of its extensive fixed-capital network (exemplified particularly by the railroads)—which could easily be financed by portfolio sources from areas other than the United States; that in a sense the greater involvement in trade and capital flows with the United States is evidence of strength which permits Canada to take advantage of its opportunities vis-à-vis this country as it could not do through the 19th and early 20th centuries.

Yet there is apprehension lest the control of important industrial sectors of Canada by large parent companies in the United States lead to policies (pricing, marketing, production, research, etc.) not as beneficial to Canada's economy, either in the short or long run, as those of domestically controlled enterprises might be; lest the increased dependence upon United States markets, for imports and particularly for exports, make Canada more sensitive to the vagaries of United States economic policy; lest the ties with international labor unions in the United States impose policies (with respect to wage rates, conditions of work, etc.) that are not as beneficial to Canada as they might be if the unions were domestically centered. And definite recommendations are made for greater employment of Canadians by foreign-controlled corporations in high managerial and professional positions, of wider publicity of accounts and clear presentation of the distinguishable results of operations within Canada, of greater participation on boards of directors by "independent" Canadians, and of the sale to Canadian interests of substantial proportions of equity shares (about 20 to 25 per cent, see *Final Report*, p. 393); as well as for changes in Canadian tax laws and other regulations to make investment in equity capital more attractive to Canadians.

While it is difficult to weigh the elements of apprehension and economic justification in this mixed attitude toward Canada's economic relations with the United States, one can fairly characterize it as cautious; or perhaps even better, as responsive (although not explicitly) to noneconomic factors and hence reluctant to accept fully the economic arguments—particularly when they cannot be thoroughly tested (which is usually the case). This attitude emerges most clearly in connection with Young's monograph 27, on Canada's commercial policy, which presents a clearly and cogently reasoned case for

free trade. Its main argument, in the author's words, is: "In general and over the long run, increases in protection can be expected to lead to economic losses and decreases in protection to economic gain for the country as a whole. This follows not only from the direct effect the Canadian tariff has on the Canadian economy, but also from the effect Canadian commercial policy has on the treatment accorded this country's exports" (p. 160). In its brief introduction, the Commission is somewhat apologetic about Young's "more abstract case for free trade . . . than perhaps some people would expect or think justified in a staff study for a Royal Commission" and then adds: "Understandably, the Commissioners have been more concerned with tariff and commercial policy in the light of the existing structure of the Canadian economy under the conditions and circumstances of today and those which we foresee in the future than we have been with theories which in themselves involve certain assumptions and preconceptions and which are, therefore, subject to different interpretations when applied in practice" (*ibid.*, front page). In the *Final Report* the Commission concludes that it would not be wise for Canada to embark upon any general program of tariff reductions on a unilateral basis, while "the impetus which has been given by the United States to a policy of freer trade is now virtually exhausted" (p. 440).

The difficulty in accepting the position indicated—in respect to commercial policy, foreign involvement, and other aspects of relations with other countries—does not lie in what may to some seem to be excessive caution, and to others excessive nationalism. Given the variety of noneconomic factors —political, sociological, cultural—that play on decisions concerning international economic relations, to claim the dominance of the economic argument and analysis is hardly warranted (and Young, incidentally, does *not* do so). The difficulty is rather that the references to these other factors are so general and vague that no firm ground for analysis and testable judgment is provided. What is meant by the "light of the existing structure of the Canadian economy"? Does it mean that no changes are desirable; or that rapid changes are undesirable, and if so how rapid? Given the account in monograph 29 of Canada's enterprises "controlled" from abroad, which reveals no serious defects in policy from the standpoint of Canada's economic growth, what specific useful purpose would be achieved by a greater share of Canadian equity holdings in such enterprises? This is not to say that there are no good answers to these questions; the point is rather that none have been given. A more explicit consideration of at least some of the noneconomic factors that lurk behind the scene might have helped.

At the risk of unwarranted speculation, one might suggest that greater economic "dependence" of Canada upon the United States, a neighbor over ten times its size, whose political decisions and cultural patterns may often not be to the taste of the Canadian community, despite obvious economic gains, involves some psychological costs. The relevant factors are cultural, socio-psychological, and political, rather than economic. Yet it is not impossible to note them explicitly, and even consider some uses of economic resources that could be expected to compensate by encouraging cultural and political leadership in Canada in respects and directions in which the United

States may be deficient. Of course, the Commission, having been instructed to deal with *economic* prospects of Canada, may have considered itself barred from discussing these noneconomic forces and factors. Yet in the case of international economic relations this limitation may be a serious disadvantage; and the refusal to deal more explicitly with the noneconomic factors may encourage a greater unwillingness to accept the purely economic arguments than might otherwise be the case.

IV. *Concluding Comments*

Several complexes of topics have been referred to only incidentally in the Commission's inquiry, and have not been treated with the fullness that they warrant.[7] The first is the mechanism, immediate determinants, and prospective implications of the substantial price rise that occurred in the years following the second world war. A rise of 60 per cent over a decade, with the differential impact that it must have had on the various groups in the community and sectors in the economy, accompanied by the rise in output and productivity that provides the basis for so much extrapolation into the future, would seem to call for full-scale analysis and for more detailed consideration of policy tools. There is a brief discussion in the *Final Report* (pp. 428-33) of the means of dealing with inflation, emphasizing the primacy of monetary measures in combination with fiscal policy and some selective controls; but the discussion is too brief and general. There is no systematic study of the price movements, over-all and differential; no full-scale analysis of their determinants or mechanism; and, as already indicated, little attention is paid to price prospects in the projections.

Second, while extensive discussion of the distribution of national product, labor force, and capital by industrial source and attachment is provided, almost no attention is paid to the distribution of incomes by type, among various occupational-industrial-economic status groups, and among different size-of-income groups. Granted that the basic data may be inadequate in Canada, as they are in the United States and other countries, yet Canadian national accounts and other sources contain sufficient data—on the distribution of income by type (compensation of employees, income of individual entrepreneurs, property incomes, etc.) combined with the industrial distribution—to shed

[7] After this review was completed, William C. Hood kindly supplied proofs of the one still unpublished monograph (No. 3 in the Appendix). The monograph is partly a detailed review of the financing of economic activity in Canada during the period from 1946 through 1956—with only the broader aggregates for 1955 and 1956; partly an appraisal of the operation of the capital market during the same period. The basic data are a set of money-flow estimates (called national transaction accounts), designed as a supplement to the national income accounts. The resulting matrix is highly elaborate, and the tables that provide the annual entries for the cells from 1946 through 1954 are a rich mine that could be quarried for years. Part II summarizes the conceptual structure of the accounts and reviews the evidence on financing for the economy as a whole for four separate periods: 1946-48, 1949-51, 1952-54, and 1955-56. Part III deals with financing of individual sectors—consumer finance and business finance (including agriculture, small business, and nonfinancial corporations); Part IV, with financial institutions (life insurance companies and other selected intermediaries, the banking system and the money market).

This major study will be reviewed separately in a forthcoming issue of the *Review*.

some light on the flow of incomes to various economic groups in the community. The combination of data on personal income taxes with those on flow of personal income to individuals and households and information provided by recent sample studies of distribution of income among nonfarm families should make possible some measure of the levels of and changes in the shares of upper-income groups. It need hardly be emphasized that in a free society, the levels of and changes in relative compensation of factors and groups in the community affect the relative supply as well as the structure of resources; and the neglect of explicit study of changes in relative compensation, past and prospective, seriously limits both the analysis of the past and the projections.

Third, no systematic and detailed analysis of government revenues and expenditures is provided. In its *Final Report,* the Commission indicates that it deliberately omitted consideration of "the whole question of the continuing relationship of the federal, provincial and municipal levels of government, and the tax revenues which each should have in order adequately to carry out its responsibilities," and explains the omission as due to the desire not to conflict with the study of this topic "by the officials of the respective governments preparatory to a Federal-Provincial Conference on the subject" (p. 455). Whatever the reason, there is no study of government revenue collections and other means of financing government activity, and their possible impact upon production, spending, and saving incentives in the private sector; nor is there any analysis of government expenditures, and their possible contributions to the various groups in the community or to the various sectors of the economy. The sheer quantitative weight of government activity, so substantially increased in recent decades—as evidenced by a rise in the share of government purchases of goods in gross national product from about 10 per cent in 1927-29 to 18 per cent in 1953-55, and projected to almost 17 per cent in 1979-81 (monograph 2, Table 7.4, p. 318), seems to warrant a detailed and explicit analysis.

Fourth, and perhaps most important, no systematic comparison of supply with needs, of growth with what growth is supposed to serve, has been attempted—either in the analysis of the past record or in the projections (except in the few cases, like that of social capital, where projections were directly based on needs). Yet it may legitimately be asked whether the growth that occurred in the past fell short of apparent needs in some specific areas or in some broad sectors; and whether the projected growth, if it is realized, will be a tolerable approximation of supplies to the range of the needs of the community as reasonably envisaged. Without such a comparison, one naturally wonders what growth is for, and whether the frequently cited statistical rate represents *adequate* growth for the major types of individual and collective needs.

Associated with these omissions, and also in part with the basic assumptions underlying the projections, is an impression of a kind of "problemlessness." Presumably, one purpose of the inquiry, in appraising Canada's prospects of growth, was to ascertain whether there were any major obstacles to growth at what might be considered satisfactory rates and with satisfactory patterns;

and to explore policies to deal with such problems. The *Final Report* gives the impression that no serious problems exist; and policy recommendations are specific on minor issues but are rather general when they touch upon a major problem like inflation. To be sure, there are recommendations for: an energy commission to guide policies in that field; an accelerated depreciation rate and other taxation measures to induce greater investment of domestic savings; changes in rules governing investment by financial intermediaries to assure better performance of capital markets; greater "integration" of foreign-controlled corporations, by measures already noted above; more imaginative treatment of the problems of growing urban conglomerates; raising the pay scales in education and other community services; immigration, particularly of skilled labor; straightening out some anomalies in tariff structure and administration. But all these seem minor, in comparison with some major problems that have been ignored.

The gravest problem of Western civilization, the danger of an all-out conflict, is removed by a general caveat; and the related potential problem, of the possible burden of defense outlay on the economy, is also removed by the more specific assumption which points to a government defense expenditure by 1979-81 of less than 4 per cent of gross national product, compared with 7 per cent in 1953-55 (monograph 2, Table 7.4, p. 318). Moreover, the share of government nondefense expenditures is assumed to rise from 11 per cent of gross national product in 1953-55 to only 13 per cent in 1980. The problem of inflation is, as already suggested, largely omitted from the analysis of the past, and removed by assumption from the projections. Any problems connected with income distribution among various groups in the country are, also as indicated, omitted from the analysis and not explicitly brought into the projections. And without systematic comparison of needs and supplies, the adequacy of past growth cannot be tested nor can any problems connected with the failure to supply some needs in the future be discussed. All that we are left with are essentially production problems, in the narrow sense of the word; and given the backlog of useful knowledge, the capacity for accelerated increase in the future, Canada's endowment with natural resources as presently seen, and some plausible growth in the United States, these are bound to be relatively minor.

These observations are not intended to be critical of the Commission's inquiry. They reflect the true situation, viz., that in rich developed countries (like Canada, the United States, and a number of others) the economic problems raised by consideration of growth prospects in terms of supply of factors and demand for products are relatively minor. The major problems are in the realm of international relations—with the Communist and underdeveloped countries; in control of inflation and its impact on the differential position of various groups; and in the use of the government sector, not only for the short run (which is not directly considered in the inquiry) but also to offset any possible *specific* deficiencies of the market mechanism and of the private sector in supplying commodities and services to satisfy some socially important needs.

The comments in this review are perhaps too demanding in their emphasis

on the omissions in the inquiry; and may perhaps understate the value of the analysis and the discussion in the *Final Report* and in the monographs. I would be the first to regret it if the review left this impression, and failed to convey an appreciation of the usefulness of the Commission's published results as a reference series of wide scope and as a valuable attempt at a systematic translation of the analyzed record of the past into a projected, if oversimplified, future.

Two comments are appropriate in conclusion. First, the period over which the studies were completed was very short. The Commission was set up in mid-1955, most of the monographs appeared by early 1957 after a period of about a year and a half, and all but one and the *Final Report* were published by the end of 1957. This is an amazingly short period in which to prepare and publish a series of monographs so impressive in coverage and content. While the brevity of the period may account in part for some of the omissions noted above, it is positive proof of the remarkable production efficiency of this research project.

Second, much of the value of the Commission and its efforts lies in the systematization of knowledge that already exists about Canada's economy, and particularly in the spread of such knowledge over wider circles—a process of education. The enrichment of widely held economic knowledge, the provision of a more balanced framework within which policy decisions can be made, is potentially a more useful service—in purely "practical" terms—than any specific policy recommendations that may be made. And this service can remain effective and increase in value only if the effort is continued and extended. We hope that the present inquiry will be the first in a series, to be followed by others at not too distant intervals—in which the same and additional aspects of Canada's economy will be treated in systematic relation to each other; and that continued wide use of the results of the present inquiry will be made by scholars and students of economic affairs—a use these results richly deserve.

APPENDIX

Classified List of Publications of the Royal Commission on Canada's Economic Prospects

All publications are by Edmond Cloutier, Queen's Printer and Controller of Stationery, Hull or Ottawa. The dates of completion are indicated following each title, publication being in the same or following year. No prices are given.

Preliminary Report, Dec. 1956, 142 pp.
Final Report, Nov. 1957, 509 pp.

A. Studies of Basic Aggregates, Projections, and Components by Type of Use:

1. J. M. Smith, *Canadian Economic Growth and Development from 1939 to 1955*, May 1957, 80 pp.
2. W. C. Hood and Anthony Scott, *Output, Labour and Capital in the Canadian Economy*, Feb. 1957, 513 pp.

3. W. C. Hood, *Financing of Economic Activity in Canada,* including *A Presentation of National Transaction Accounts for Canada, 1946-1954,* by L. M. Read, S. J. Handfield-Jones and F. W. Emmerson, to be published in 1959, about 700 pp.
4. D. W. Slater, *Consumption Expenditures in Canada,* May 1957, xi + 198 pp.
5. Yves Dubé, J. E. Howes and D. L. McQueen, *Housing and Social Capital,* Jan. 1957, 164 pp.
6. R. D. Howland, *Some Regional Aspects of Canada's Economic Development,* Nov. 1957, xi + 302 pp.

B. Industry Studies:

7. W. M. Drummond and W. Mackenzie, *Progress and Prospects of Canadian Agriculture,* Jan. 1957, 424 pp.
8. John Davis, A. L. Best, P. E. Lachance, S. L. Pringle, J. M. Smith, D. A. Wilson, *The Outlook for the Canadian Forest Industries,* March 1957, 261 pp.
9. The Department of Fisheries of Canada and The Fisheries Research Board, *The Commercial Fisheries of Canada,* Sept. 1956, 193 pp.
10. John Davis, *Mining and Mineral Processing in Canada,* Oct. 1957, ix + 400 pp.
11. John Davis, *Canadian Energy Prospects,* March 1957, 392 pp.
12. Urwick, Currie Ltd., *The Nova Scotia Coal Industry,* June 1956, 34 pp.
13. The Bank of Nova Scotia (Dr. Lucy Morgan), *The Canadian Primary Iron and Steel Industry,* Oct. 1956, 104 pp.
14. National Industrial Conference Board (Canadian Office), *The Canadian Primary Textiles Industry,* July 1956, 105 pp.
15. John Davis, *The Canadian Chemical Industry,* March 1957, 182 pp.
16. D. H. Fullerton and H. A. Hampson, *Canadian Secondary Manufacturing Industry,* May 1957, 274 pp.
17. The Sun Life Assurance Company of Canada, *The Canadian Automotive Industry,* Sept. 1956, 119 pp.
18. J. D. Woods and Gordon Ltd., *The Canadian Agricultural Machinery Industry,* Apr. 1956, 47 pp.
19. Urwick, Currie Ltd., *The Canadian Industrial Machinery Industry,* Feb. 1956, 31 pp.
20. C. L. Barber, *The Canadian Electrical Manufacturing Industry,* Sept. 1956, 87 pp.
21. Canadian Business Service Ltd., *The Electronics Industry in Canada,* Apr. 1956, 81 pp.
22. The Royal Bank of Canada, *The Canadian Construction Industry,* Oct. 1956, 232 pp.
23. J.-C. Lessard, *Transportation in Canada,* Nov. 1956, 160 pp. (plus 29 pp. of statistical schedules, unnumbered)
24. The Bank of Montreal, *The Service Industries,* March 1956, 161 pp.

C. Studies of Foreign Trade, Investment, and Policy:

25. D. W. Slater, *Canada's Imports,* Jan. 1957, 222 pp.
26. R. V. Anderson, *The Future of Canada's Export Trade,*[1] March 1957, 338 pp.
27. J. H. Young, *Canadian Commercial Policy,*[1] Nov. 1957, 235 pp.
28. J. G. Glassco (of Clarkson, Gordon & Co.), *Certain Aspects of Taxation Relating to Investment in Canada by Non-Residents,* Feb. 1956, 64 pp.
29. Irving Brecher and S. S. Reisman, *Canada-United States Economic Relations,*[1] July 1957, 344 pp.

D. Other Studies:

30. The Canadian Bank of Commerce, *Industrial Concentration, A study of Industrial Patterns in the United States, the United Kingdom and Canada,* June 1956, 62 pp.
31. The Canadian Labour Congress, *Labour Mobility,* Sept. 1956, 11 pp.
32. The Canadian Labour Congress, *Probable Effects of Increasing Mechanization in Industry,* Sept. 1956, 87 pp.
33. The Economics and Research Branch, Dept. of Labour of Canada, *Skilled and Professional Manpower in Canada, 1945-1965,* July 1957, xiv + 106 pp.

[1] One of a series of three studies prepared under the direction of S. S. Reisman.

COMMUNICATIONS

Rent as a Measure of Welfare Change

The definitions of economic rent in current use fall easily into two categories: (1) a payment in excess of that necessary to maintain a resource in its current occupation. Thus, Frederick Benham [1, p. 227] tells us that rents are "... the sums paid to the factors which need not be paid in order to retain the factors *in the industry*." While to Kenneth Boulding [2, p. 230] it is the payment to a factor "... in excess of the minimum amount necessary to keep that factor in its present occupation." (2) The difference between the current earnings of a resource and its transfer earnings[1]—the latter term signifying its earnings in the next best alternative use [1, p. 328]. For instance, Paul Samuelson [6, p. 593] says, "... we should term the excess of his income above the alternative wage he could earn elsewhere as *a pure rent*." Similarly, for George Stigler [7, p. 99] the rent of a factor is "... the excess of its return in the best use over its possible return in other uses? ..."[2]

While the first type of definition is, as we shall see, unavoidably ambiguous, the second type is yet more inadequate. Among other things it would require that, in the choice of occupation, men were motivated solely by pecuniary considerations.

I. *A Measure of Rent as an Economic Surplus*

For the purpose of revealing ambiguities in the existing definitions of economic rent and of demonstrating the logic of the proposed definition, we shall find it no less convenient and a good deal more suggestive to take our bearings from a more generalized version of the traditional theory of consumer's choice.

Rather than maximizing the utility function $W\{u(x_1, \ldots, x_n)\}$, over the range in which $\dfrac{\partial W}{\partial x_r} > 0$ for all x_r, subject to the usual constraint $\Sigma p_r x_r = Y$, where Y is the individual's income [cf. 3, p. 305], we require our individual, in possession of given resources, or assets, to maximize such a function subject to $\Sigma p_r x_r = 0$. At least one of the x's is negative in order to indicate a quantity supplied per period by the individual of a good or service and, of course, at least one of the x's is positive to indicate a quantity demanded per

[1] To impart precision to this measure of economic rent the period of adjustment should be specified, as should, also, the area of comparison—within the industry, region, country, or within the world as a whole. But since the inadequacy of this definition of rent prevails irrespective of these distinctions, I shall make no further mention of them in this paper.

[2] In all these cases the writers appear to be using "factor" in the sense in which I shall use the term "resource." And though, generally, I prefer to reserve the term factor for the productive service of the resource, it will avoid possible confusion if instead I adhere to the term productive service.

period of a good or service. The suggested constraint expresses nothing more than the proposition that, in all circumstances, the individual's current earnings are equal to the current value of his expenditure.[3] It is a significant amendment, however, because it brings to the fore the notion of simultaneous determination of the individual's allocation of his productive services and of his earnings in response to a given pattern of prices: an obvious point perhaps, but one frequently ignored in the analysis of the individual's demand and supply curves.

Maximizing the utility function subject to our new constraint, we derive the well-known equilibrium condition $\frac{\partial W}{\partial x_r} = \lambda p_r$ (λ being identified as the marginal utility of income) for all goods and services whether their magnitudes are positive or negative—whether, that is, they are demanded or supplied by the individual.[4] Or, dispensing with utility, we can write $\frac{\partial x_i}{\partial x_j} = \frac{p_j}{p_i}$ for any i and j.

It should be apparent that, although the substitution effect may be defined in the customary way, there can be no income effect, $\frac{\partial x_r}{\partial Y}$, since there is no necessary correspondence, using our new constraint, between changes in the individual's welfare and changes in his income, real or money. For with the new constraint, money income, Y, is no longer held constant; it is determined along with all the other variables. It may increase, remain unchanged, or diminish, with an improvement in the individual's welfare. In its place, therefore, we derive a *welfare* effect, $\frac{\partial x_r}{\partial W}$. In consequence, the effect on the quantity bought or sold of any chosen good or service of a given change in the set of prices is divided into a substitution effect and a welfare effect.

The implications of this less-restricted formulation, though straightforward enough, are worth recording. A change in the price of any good or service—whether it is supplied or demanded by the individual—changes, in general, the quantities of all goods and services which the individual buys and sells. Consequently it changes the value of his earnings and expenditure. A search for a useful definition of an "incentive good" might begin with the implication that a fall in the price of any consumed good will, *inter alia*, increase or reduce the amount of work done by the individual as a result of the operation of the welfare effect. But this will not be pursued here.

[3] Strictly speaking his spending is equal to current earnings *less* current saving *plus* current dissaving. This could easily be allowed for without any modification of our conclusions. Over time, if his assets grow, his demand for goods and his disposal of productive services will, of course, alter. This problem is, however, common to all such static analysis.

[4] Since we restrict ourselves to the range in which the marginal utilities of all goods and services are positive, the acquisition of goods or services adds to the individual's total utility while the supply of goods and services from the individual's assets or resources subtracts from his total utility. Corresponding to the equilibrium conditions for goods purchased, the marginal utilities of the productive services supplied to the market are proportional to their corresponding supply prices.

Having extended the customary confines of the theory of consumer's choice we may now develop the argument largely in terms of two or three goods or services, but deriving from our hypothesis a more symmetrical construction of the individual indifference map. Since $\Sigma p_r x_r = 0$, the price hyperplane passes through the origin of an n-dimensional indifference map and is nega-

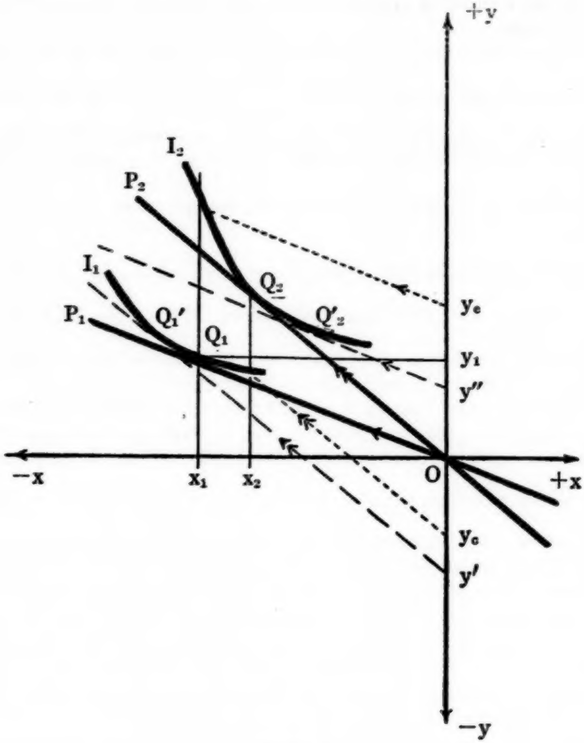

FIGURE 1

tive in slope with respect to all the axes. This means that in order to acquire (surrender) more of one good or service, other goods or services must be surrendered (or acquired).

A two-dimensional cross section of this indifference map is represented in Figure 1. Any distance Ox to the right of the origin measures amount per unit period of x acquired by the individual. Any distance Ox to the left of the origin measures the amount of x per period given up by the individual. Similarly, Oy above the origin measures the quantity of y taken, and Oy below the origin, the quantity of y given up. Inasmuch as rent partakes of the nature of

a surplus, and is to be measured in exactly the same way as consumer's surplus is measured, it is advantageous to consider in some detail the simple problem of the individual supplying x, say a single type of productive service, "labor," to the market in return for which y is demanded. Thus, we operate in the north-west quadrant of the figure. And though we may not do so in an n-dimensional treatment of the problem, confined as we are to two dimensions we may find it convenient to regard y as all other goods at fixed prices, the only price which alters being the price of x, labor.[5] We now seek a precise measure of the difference in welfare resulting from alternative supply prices of labor.

If we construct a price line P_1 passing through the origin and tangent to Q_1 on the indifference curve I_1, the individual is represented as in equilibrium, giving up Ox_1 of labor and acquiring in exchange Oy_1 of income y. We now perform the familiar Hicksian experiment in order to have the supply effects on all fours with those of demand. The price of x is now increased from p_1 to p_2, the individual's new equilibrium being at Q_2 on the indifference curve I_2. The change in equilibrium positions consequent upon the change in the price of labor may be divided into the substitution effect, Q_1 to Q_1', and the welfare effect, Q_1' to Q_2 (or alternatively the welfare effect Q_1 to Q_2' and the substitution effect Q_2' to Q_2). Although the welfare effect can, of course, go either way, it should be noticed that a positive welfare effect on x, implying an increase in the *demand* for x, constitutes a reduction in its supply, which is to say that a positive or "normal" welfare effect of a rise in the supply price of labor, or in the supply price of any good or service, is that of a reduction in the quantity supplied by the individual. The "backward-bending" supply curve of labor is, then, the outcome of a strong positive, or normal, welfare effect, and not a negative, or perverse, welfare effect.

Suppose we are now to measure the increase in welfare following a rise in the price of x to p_2, we may follow Hicks' practice [4, pp. 69-82] and distinguish between two preliminary measures: the compensating variation (CV), and the equivalent variation (EV). The CV is the amount of y which, following a change in the price of x, has to be given to or taken from the individual in order that his initial welfare—indicated by the indifference curve I_1 in Figure 1—remain unchanged. In this instance, the individual's welfare being

[5] This construction, and its later elaboration, are, I believe, to be preferred to the more common leisure-income diagram apart from the fact that the present diagram is derived directly from the more general condition in which the individual chooses to supply a combination of various goods and productive services to the market in amounts which depend upon the current set of prices: (1) Giving up leisure, a homogeneous good, does not have the same connotation as providing various kinds of services each of which requires a different skill and entails a different degree of hardship for the individual. (2) We need not evoke the artifice of a fixed amount of the good, leisure, say 24 hours a day, with the rather awkward result that an improvement in welfare may be represented along one axis as equivalent to more than 24 hours of leisure a day. In the construction used here, the shape of the indifference curves acts to limit the supply of any productive service furnished to the market, and our measure of welfare changes is in terms only of the good, y. Finally (3) the indifference map used here is the correct prior construction to that useful textbook diagram in which a downward-sloping line crosses the price-axis, to the right of which is represented the demand schedule and to the left, the supply schedule.

improved as a result of the price change, Oy' measures the CV. For if Oy' were taken from his income he could still maintain his initial welfare position on I_1, given that the higher supply price P_2 is available to him. The EV, on the other hand, is the amount of y which has to be given to, or taken from, the individual to ensure that he reaches the new level of welfare when the change in price does not apply to him. Since in this instance the increment in welfare is positive he is to receive a money equivalent. If he receives Oy'' he can just reach I_2, the new level of welfare, with the old price P_1.

The concept of rent as an economic surplus, it is suggested here, should be measured as a CV or an EV in a manner symmetrical in all respects with the concept of consumer's surplus. In the example above, it arises as the difference in welfare experienced by the individual from the rise in the supply price to P_2, P_1 being regarded as the most preferred alternative open to him.[6] The rent obiviously becomes larger the lower the initial supply price P_1. In the limiting case, P_1 will be a no-transactions price tangent to an indifference curve at the point where it crosses the vertical axis.

Since the current definitions treat rent as a surplus which may be appropriated without any effects on the supply of the individual's productive services in his current occupation, it is important to observe that in all cases in which the individual is made to pay or to receive compensation equal to the measures of rent suggested, the amount of the productive service he will then offer will differ from that which he originally supplied at the current price. For example, if, having reached Q_2 in Figure 1, he is made to pay the full CV, equal to Oy', he will no longer continue to supply Ox_2 of labor. Instead he will supply the amount indicated by the equilibrium point Q_1'—a larger amount than before if x is normal.

Finally it may be instructive to remove the restriction of a single occupation in our analysis and to consider briefly the case of the supply of productive services to two alternative occupations, A and B, in which, although the individual might choose to work part-time in each if that were feasible, he is obliged, owing to institutional arrangements, to work entirely in the one occupation or the other.

In Figure 2, a three-dimensional indifference map with a vertical y-axis and two horizontal axes, a and b, crossing at right angles, we cut a vertical slice along the negative ay plane and along the negative by plane as far as the

[6] Though we are working with a single productive service, labor, the notion and the definition of economic rent may, just as in the analysis of consumer's surplus, be extended to several services with obvious modifications. If, for example, the individual is providing two services, x_1 and x_2, then a rise in the supply price of both services yields a CV rent which is the maximum he is willing to pay—prices of all goods and services other than those of x_1 and x_2 remaining unchanged—rather than forego these higher prices. This measure remains the same, as we might expect, if we measure each in turn and add them: the rent when the price of x_1 rises, all other prices, including that of x_2, being constant, *plus* the additional rent when now the price of x_2 rises, all other prices remaining constant with x_1 unchanged at its new price.

This argument is symmetrical with that of Hicks on consumer's surplus [4, pp. 178-79], but the generalization in the conclusion of this paper goes further than Hicks'.

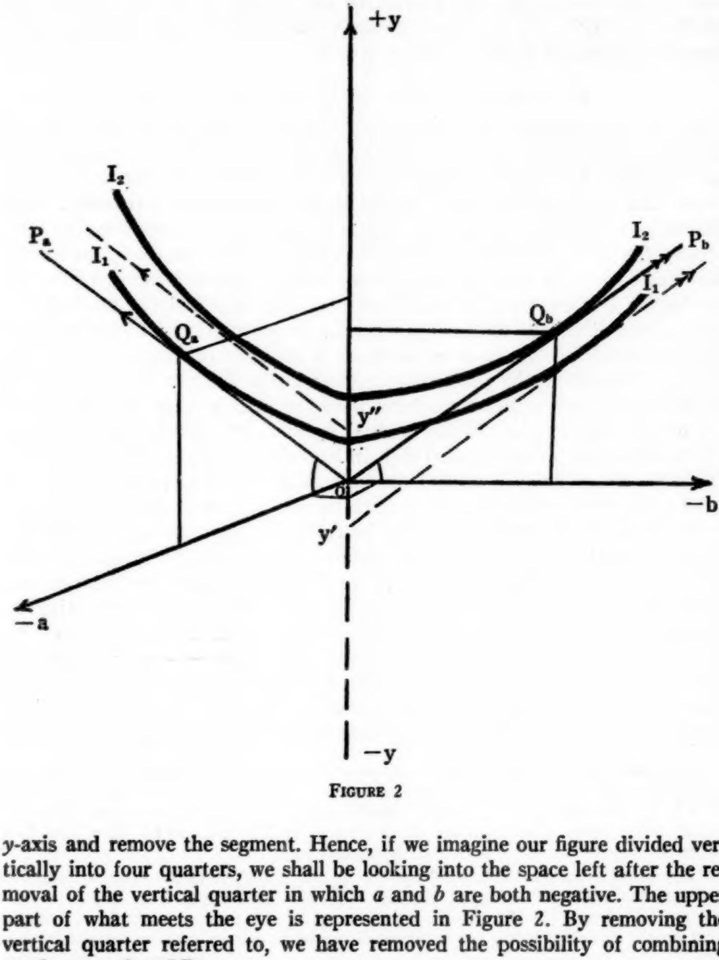

FIGURE 2

y-axis and remove the segment. Hence, if we imagine our figure divided vertically into four quarters, we shall be looking into the space left after the removal of the vertical quarter in which a and b are both negative. The upper part of what meets the eye is represented in Figure 2. By removing the vertical quarter referred to, we have removed the possibility of combining employment A and B.

Despite the fact that both the rate of pay and the resultant earnings are higher in A than in B, the individual chooses to supply his services to B, his equilibrium there being at Q_b on the indifference surface I_2 compared with the alternative equilibrium position Q_a on I_1. Nonetheless, he enjoys a positive economic rent in the lower-paid occupation B which can be measured by the *CV*, Oy'—the maximum he is prepared to pay to remain in B when A, at the

existing wage-rate, is the only alternative open to him. It can also be measured by the *EV*, Oy''—the minimum the individual must be paid in order to induce him to transfer his services from B to A.[7]

II. *Comparison with the Marshallian Concept*

Let us now compare our results with Marshall's concept of rent. Though the *Principles* do not contain a formal definition of rent, the sense of most of Marshall's dicta on the subject points to a definition of rent as a surplus above that necessary to elicit the productive services of a resource.[8] This Marshallian definition, essentially that of category (1), suffers from the same imprecision as his definition of consumer's surplus.[9] For one thing, the surplus was treated as if it could be taxed away without affecting the supply of the productive service, which is manifestly false on our analysis. Once this is granted the difficulties are easily perceived. In order to persist with the Marshallian definition we have to interpret it to have reference to some unchanged amount of the productive service; either (a) the amount supplied in the equilibrium position resulting from the price change (Ox_2 in Figure 1), or (b) that supplied in the original equilibrium position (Ox_1 in Figure 1). In either case we are saddled with an improbable and cumbersome measure inasmuch as we have to compel the individual to supply an amount of the productive service other than that which he would freely choose. For instance, if we adopt (a) we derive what may be called, for convenience, the Marshallian *CV*, equal to Oy_c. It represents the maximum amount of money the individual would surrender in order to retain P_2 if at the same time he were constrained to provide no more than Ox_2 of productive service—Ox_2 being the amount supplied at P_2 when he is free to choose. As we should expect, the restriction on his choice of quantity reduces the maximum he is prepared to pay for the privilege of the P_2 price. In a like manner, if we adopt (b), the Marshallian *EV*, we are left with a measure Oy_e. It is larger than the *EV* proper, Oy'' since the minimum payment to him must be greater if he is now

[7] In all cases in which institutional arrangements preclude a combination of occupations —the individual having the choice only of putting the services of his resources entirely in A or in B—the coincidence of the four definitions and the Marshallian measure of rent no longer follow from a zero welfare elasticity, for the *EV* and the *CV* now arise from different cross-sections of the individual indifference map.

[8] In particular see pp. 155-62, and pp. 427-30 [5]. Elsewhere in the *Principles* Marshall talks of the additional earnings resulting from superior abilities as a surplus or rent [5, pp. 577-79 and 623-27]. What part of the additional earnings might be regarded as rent on the definition attributed to Marshall cannot be known without determining first what part of the additional earnings is necessary to attract the resource into that occupation. From the point of view of the firm, however, the additional payments for superior abilities must appear as efficiency payments.

[9] [5, p. 124] Marshall was, of course, aware of the slippery nature of his consumer's surplus (though not, apparently, of his economic rent) and tried to cover himself by specifying a change in the demand price for a particular good whose real-income effect was so small in relation to the individual's budget that the marginal utility of money income could be taken, for all practical purposes, as constant. The trouble with this is that it is double-edged: ambiguity is reduced by reducing the significance of what is being measured. Ambiguity disappears entirely only when the price change under consideration becomes zero and there is nothing left to measure.

compelled to provide the original quantity of productive services Ox_1 at the original price P_1 when his welfare is increased from I_1 to I_2. There is obviously nothing strictly illogical about such definitions, but on the grounds of plausibility and convenience they are to be rejected in favor of the *CV* and *EV* proper.

If, on the other hand, a Marshallian *measure* of economic rent is taken to be the area above the supply curve of the services of the individual's resource[10] —a measure which seems to correspond with the category (2) definition if the individual's supply curve represents maximum earnings of successive increments of productive services in alternative uses—for this measure to be of any use requires (i) an upward-sloping supply curve, and (ii) exclusion of nonpecuniary considerations. Clearly this Marshallian measure, which is popular in textbooks, is inadequate since it represents no more than a first derivative of the locus of price-quantity equilibria of an indifference map. Nor is this derivative necessarily upward-sloping; it may be backward-bending in contrast to the *marginal* indifference curves which will always be upward-sloping. It appears yet more unsatisfactory if the restriction to pecuniary considerations is removed. We may then discover that differences between the earnings of the resource in its current occupation and those of the relevant alternative occupation are negative, a tribute to the individual's preference for his present occupation.

Hicks has done some admirable work in tracing the relationships between Marshall's definition of consumer's surplus, Marshall's way of measuring consumer's surplus (the area under the individual's demand curve), and the two precise measures *CV* and *EV* which were initially suggested by his indifference curve analysis. Important as these contributions were in clarifying our ideas on this tangled subject, it can be held that the tracing of these precise relationships assumes a far greater importance on the neglected supply side. For it is surely just there that we cannot reasonably suppose that a change in price has negligible effects on the welfare of the individual inasmuch as the supply of any one of his productive services enters significantly into his budget. To the extent it does so, the area above the individual's supply curve, especially in the case of only one productive service, is a much less reliable index of the surplus welfare than the area under his demand curve for any one good.

In the special case in which the welfare elasticity of the supply of x is zero there is a coincidence of the *CV*, *EV*, Marshallian *CV*, Marshallian *EV*, and the Marshallian measure, the area above the supply curve. (This same coincidence obtains when the rent is reckoned as between the current and alternative occupations in the case in which the choice between occupations rests on a purely pecuniary basis.) While in general, a zero elasticity of supply with

[10] [5, p. 811.] Here Marshall graphically illustrates consumer's surplus and producer's surplus for an *industry*. But even if we interpret the industry's supply curve as a marginal curve, the producer's surplus could be identified with the rent of resources in that industry only under restricted conditions. On the other hand, it would hardly be inconsistent with Marshall's view of things to interpret the measurement of the individual's rent in a manner symmetrical with his suggested measurement of the individual's consumer's surplus [5, pp. 125-27] as the area under the individual's demand curve.

respect to price does not entail a zero welfare elasticity, in the particular case in which the former derives from a zero substitutibility *plus a zero welfare elasticity*, these four definitions and the Marshallian measure all come to the same thing. The zero substitutability implies no alternative uses and therefore a set of vertical *marginal* indifference curves. The zero welfare elasticity implies that all the marginal indifference curves will coincide. Ricardian land is a favorite example of a zero elasticity of supply of this sort. Its characteristic is that it has only one use, say wheat production. As a consequence of this characteristic (i) it cannot move elsewhere in response to changes in relative prices (zero substitution effect), and therefore (ii) *all* of a given acreage of land of uniform quality is brought into wheat production in response to any positive price per acre (zero welfare effect).[11]

III. *Conclusion*

Little further reflection is required to recognize that consumer's surplus and economic rent are both measures of the change in the individual's welfare when the set of prices facing him are changed or the constraints imposed upon him are altered. Any distinction between them is one of convenience only: consumer's surpluses have reference to demand prices, economic rent to supply prices. Furthermore, no consideration of logic precludes our measuring the individual's gain—in terms either of the *CV* or the *EV*—from, say, a simultaneous fall in the price of a good bought and a rise in the price of a service provided.

Indeed, in general, if any one, several, or even all prices change for the individual, some demand prices and some supply prices rising, others falling, the resulting change in the individual's welfare can, in principle, be measured by either of our definitions. The *CV* is an exact measure of the transfer, to or from the individual, following a change in the set of all prices, in order to maintain his initial level of welfare. In this case the amount transferred is measured in terms of any one good, in combinations of various goods, or in a combination of all goods dealt in, always using the *new* set of prices. This is quite possible since, given a set of prices, the amount of any one good is equivalent in value to various combinations of some particular goods or of all goods. More usefully, an amount of money calculated at the given set of prices will suffice to measure the *CV*.

On the other hand, the *EV* is an exact measure of the transfer necessary to bring the individual's level of welfare into equality with what it would have been if he were not, as he is, debarred from the new set of prices. The amount of the transfer is now calculated at the *old* prices, and may be expressed in money or in any combination of goods at these prices.

E. J. MISHAN*

[11] The indifference curves in this special case would all be horizontal (signifying zero elasticity of substitution) up to a distance representing the maximum supply of productive service from the given resource. At this distance they would all become vertical and, hence, coincide. Rent however measured would, on this vertical limit, be equal to the vertical distance between the two price lines in question.

* The author is assistant lecturer at the London School of Economics. He wishes to acknowledge his indebtedness to S. A. Ozga for valuable criticisms and suggestions.

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Shifts in Factor Payments and Income Distribution

The major shift in the inequality of income from 1929 to the end of the second world war and its relative stability since that time[1] can be explained in part by the corresponding pattern of, first, the major shifts in, and later, the relative stability of, the various major types of income payments. The following method measures the quantitative importance of the effect of factor-payment changes on income redistribution. The procedure is illustrated in Table 1. The 1952 distribution is presented in the top section of the table in conjunction with the percentage importance of the major types of income. The lower section of the table shows what the 1952 distribution could have looked like had the major types of income had the same percentage importance that they had in 1929.

The only 1929 data used in the redistribution technique are the 1929 major-type percentages presented in Table 1 in the last row.[2] These are multiplied by the 1952 total income of $212.9 billion to obtain each major-type dollar total (for the 1929 distribution) shown in the next to last line of Table 1. Each major-type dollar total is then distributed among the four income classes on the same percentage basis as that shown by the 1952 distribution for that major type. Because of the nature of the data, it was not only necessary to assume constant dispersion within each major type of income, but to assume that the number of families in each of the four income classes would remain the same after the redistribution.

The distribution of 1952 total family personal income by income classes (based on the 1929 major-type distribution) is now simply obtained by adding, for each income class, the incomes of the different major types. The percentage distribution by income classes is derived from these totals.

The 1929 major-type distribution is more concentrated because dividends,

[1] See Selma Goldsmith, "Size Distribution of Personal Income," *Surv. Curr. Bus.*, Apr. 1958, *38*, 11-12 and Lee Soltow, "The Trend Movement in the Income Distribution in Wisconsin for a Twenty Year Period," *Rev. Econ. Stat.*, May 1957, *39*, 223-25.

[2] The available major-type income-class data for 1952 presented in Table 1 are for nonfarm multiperson families. The total of $212.9 billion is somewhat less than total nonfarm personal income of $261 billion. Major-type percentages which are comparable for both 1952 and 1929 are available only for all nonfarm persons. Major-type percentages for nonfarm multiperson family income in 1929 were obtained by: (1) computing, for each major type of personal income, the ratio of the 1952 percentage to that of 1929 for the income of nonfarm persons; (2) applying each of these ratios to the corresponding major-type percentage for 1952 multiperson family income.

TABLE 1—DISTRIBUTION OF PERSONAL INCOME FOR NONFARM MULTIPERSON FAMILIES BY MAJOR TYPE AND BY INCOME CLASS FOR 1952 AND ITS REDISTRIBUTION BASED ON 1929 MAJOR-TYPE DISTRIBUTION
(dollar data in billions)

1952 Family Personal Income (before taxes)	Nonfarm Families (per cent)	Total Family Personal Income		Wages and Salaries (dollars)	Business, Professional Income (dollars)	Dividends (dollars)	Interest and Rental Income (dollars)	Transfer Payments (dollars)
		(per cent)	(dollars)					
Under $4,000	32.5	14.7	31.3	23.1	.8	.5	3.0	3.9
$4,000–$5,999	33.3	27.4	58.3	47.6	3.4	.6	4.1	2.6
$6,000–$9,999	25.5	31.4	66.9	53.4	5.5	1.1	4.9	2.0
$10,000 & over	8.7	26.5	56.5	30.0	12.9	6.6	6.5	.5
Total	100.0	100.0	212.9 (100%)	154.1 (72.4%)	22.6 (10.6%)	8.7 (4.1%)	18.5 (8.7%)	9.0 (4.2%)
1952 Redistribution based on 1929 types								
Under $4,000	32.5	13.5	28.7	19.9	.9	1.0	5.5	1.4
$4,000–$5,999	33.3	25.5	54.3	40.9	3.7	1.2	7.6	1.0
$6,000–$9,999	25.5	30.1	64.0	45.8	6.0	2.3	9.2	.7
$10,000 & over	8.7	30.9	65.9	25.8	14.1	13.7	12.1	.2
Total	100.0	100.0	212.9 (100%)	132.5 (62.2%)	24.7 (11.6%)	18.1 (8.5%)	34.4 (16.2%)	3.3 (1.5%)

Figures do not necessarily add to totals due to rounding.
Source: Surv. Curr. Bus., June 1956, pp. 10, 13; July 1956, pp. 10, 12; July 1958, pp. 4, 6.

business income, and interest and rent, the three types of income most unequally distributed, were relatively more important in that year. The mean difference[3] computed from the four income classes is $3,820 for the 1952 distribution and $4,320 for the 1952 redistribution using 1929 percentages

TABLE 2—MEAN DIFFERENCE INDEX AND MAJOR TYPES OF PERSONAL INCOME FOR NONFARM MULTIPERSON FAMILIES, 1929–57

Year	Mean Difference or Coefficient of Concentration Index (1929=100)	Percentage Importance of the Major Type in the Total Personal Income for the Year					
		Wages and Salaries	Business, Professional Income	Dividends	Interest and Rental Income	Transfer Payments	Total
1929	100	62	12	8	16	2	100
1930	99	63	11	9	16	2	100
1931	95	62	9	8	17	4	100
1932	91	63	7	6	20	4	100
1933	90	65	8	5	19	4	100
1934	92	66	9	6	15	4	100
1935	93	66	10	6	14	4	100
1936	94	65	11	8	12	4	100
1937	96	66	11	8	12	3	100
1938	93	66	11	6	13	4	100
1939	94	66	11	6	13	4	100
1940	94	66	12	6	12	3	100
1941	95	68	13	6	10	3	100
1942	94	71	13	4	9	2	100
1943	93	74	13	4	8	2	100
1944	92	75	12	4	8	2	100
1945	91	73	13	3	8	3	100
1946	90	68	14	4	9	6	100
1947	89	70	12	4	8	5	100
1948	90	70	12	4	8	5	100
1949	89	69	12	5	9	5	100
1950	89	69	11	5	9	6	100
1951	89	72	11	4	9	4	100
1952	88	72	11	4	9	4	100
1953	88	73	10	4	9	4	100
1954	88	72	10	4	9	5	100
1955	88	72	10	4	9	5	100
1956	88	72	10	4	9	5	100
1957	87	72	9	4	9	5	100

Figures do not necessarily add to totals due to rounding.
Source of Data: See Table 1 and footnote 2.

for the different types of income. Thus it might be stated that the change in the relative importance in the types of factor payments over this period is responsible for each family's income being 12 per cent closer to every other family's income.

Similarly, the mean difference for the 1952 distribution of income has been

[3] This is Gini's mean difference. See M. G. Kendall, *The Advanced Theory of Statistics*, 4th ed., London 1948, pp. 42-46.

computed using in turn the major-type percentages of each of the years 1930-57. The resulting mean differences, expressed as percentages of the 1929 figure, are shown in Table 2 along with the major-type percentages.

It must be remembered that the procedure assumes that total income for each year was the same as it was in 1952 and that the dispersion of income within each major type remains constant, so that only the change due to the shifting importance of types of payments is reflected. Differences due to changes in such characteristics as age, education, and occupation are not considered except at they are responsible for shifts in major types of payment.

It can be seen that from 1929 to 1933, with the drop in importance of the two most unequally distributed types (dividends and business income), an inequality level near that of the postwar period was achieved.[4] Half of this decrease was regained by the start of the war, largely because of the increased importance of business income. The sharp drop in interest and rent had a more limited effect since the inequality of this type is much closer to that of the inequality of total income.

A comparison of 1941 with 1947 shows decreases in the three most concentrated types, and increases in the two least concentrated types. The relative constancy of the mean difference since 1947, as shown in Table 2, reflects the fact that the percentage importance of the factors has been constant except for a shift from business and professional income to wages and salaries.

The radical shifts in importance of the major types of payments from 1929 to 1947 decreased inequality over 10 per cent; and the relative stability of major types since 1947 explains, in part, why the distribution of income has remained constant since that time.

LEE SOLTOW*

[4] The coefficient of concentration (*ibid.*, p. 45) can be used as an indication of the degree of inequality for each of the five major types in 1952. The coefficients for the 1952 data of Table 1 assuming each family received all types are: .27 for wages, .63 for business and professional income, .72 for dividends, .36 for interest and rental income, —.13 for transfer payments, and .32 for total income. The negative figure for transfer payments arises because the families in the lowest-income class receive a large per cent of transfer payments. The coefficient of concentration of total income for each year can be approximated by applying the major-type percentages of Table 2 as weights to the above coefficients.

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Interstate Apportionment of Business Income

The Supreme Court early in 1959 ruled explicitly on the scope of state jurisdiction under the Constitution to tax income from purely interstate business activity. The Court decided that states have significantly broader power than they have been exercising. A nondiscriminatory, properly apportioned state tax on net income can now be imposed on a foreign (out-of-state) business when the activities in the state constitute a sufficient "nexus." The contact necessary to sustain tax, we now learn, is decidedly less than formerly assumed. There is no doubt about the emergence—more accurately the accentuation—of a problem with economic, as well as legal and accounting, aspects.

In principle the extension of state taxes as now permitted would not be cause for concern *if* compliance costs could be kept modest and *if* a proper basis for apportionment were utilized. Yet it seems inevitable that compliance costs will often be high in relation to tax, not infrequently more than the tax itself. One method of helping minimize what is avoidable social waste would be to standardize the formula. The more states using the same formula, the more economically businesses can comply and, in fact, the more efficiently governments can administer the tax. Perhaps now that many more businesses face a problem which heretofore has involved only a relative few, significant political pressure will build up for standardization—possibly even for Congressional action.[1]

For many decades states have sought to allocate, i.e., to divide up for tax purposes, the income of a corporation or unincorporated business among the states in which the firm carries on its activities. Many formulas have been devised and have received at least implicit Court approval. There has been a long and frustrating history of efforts to get agreement on a single formula. Despite some progress toward uniformity, practice still differs widely.[2]

States utilize their freedom to prescribe the formula each prefers. Any large firm will likely find itself computing and paying tax on several bases. It now seems certain that very many more firms must comply with many more laws. The prospect of being subjected to state tax on more than 100 per cent of net income has increased. Yet this is less serious for these firms than the added cost of accounting, legal services, record keeping and storage, negotiation, and litigation. Untold numbers of small and medium-sized firms face a discouraging increase in operating costs—costs most of which for the economy are sheer waste.

The failure of states to achieve greater harmony results in part from the desire of some states to "import" income for tax purposes. Choice of one as against another allocation formula can permit just this result. For example, a state whose residents buy more manufactured goods from outside than they "export" can decree that a large part (or all) of business net income originates where the products are sold, i.e., where buyers live. The place where goods are made is held to be unimportant. Adjustment of the elements of the formula and of their weighting to take account of the nature of a state's economy can make a difference in the state's tax base. Legislatures can reach out and tax "foreigners," corporations (stockholders, consumers, employees, or whoever bears the corporation tax) who have no vote in the state. Any

[1] There does seem to be persuasive argument for national action. There *is* a public interest which extends over the whole economy. There is *not* much basis for arguing that the persons affected can protect themselves in state legislatures. How often will a small "foreign" business have any significant political influence with the legislatures of other states which now have power to tax it? The ability of businesses to protect themselves by shifting the tax to consumers is a topic requiring more analysis than possible here.

[2] The National Conference of Commissioners on Uniform State Laws did agree in 1957 on a model income-allocation statute. The American Bar Association approved. Considerable opposition developed in the business community, however, and apparently no legislature has yet (April 1959) adopted the proposal. Though undoubtedly excellent in many ways it is subject to the basic defect which I discuss below.

state is now free to decree that some condition which does not in fact create income shall be assumed for tax purposes to be the source of income. In view of current and prospective pressure for state revenue and of the invitation the Court has now extended, ambitious, even greedy efforts are not unlikely.

In such cases the rational taxpayer will try to avoid whatever brings no income but which does bring tax liability. He will wisely shift activity from one form to another if by so doing the loss of income (or the extra expense) is less than the tax saving. There is a presumption, I think, that such shifts will involve some loss of real income through poorer allocation of resources. Any gain to this or that state treasury may be bought at much greater cost to the economy.

The most widely used basis for income allocation is the "Massachusetts formula." Property, payrolls, and sales (receipts) are employed and weighted equally.[3] This formula was developed more than half a century ago. But use of the sales (receipts) factor has little—or no—economic justification. Human effort produces income. Use of property produces income. Selling and purchasing, however, do *not* produce income except as human effort and property are involved. Although selling is, of course, a part of the income-creating process, its contribution is represented not by the dollar volume of sales but by the remuneration of the human and material resources that perform the selling function. To assign sales an independent importance is to depart from economically proper apportionment. Worse still, this practice creates an inducement to arbitrary, complex, and wasteful manipulation of sales arrangements. I submit that a formula based on economic reality will accord no separate or distinct place to sales.

Assuming some possibility of standardization, what goal would best serve the public interest? What formula would have the least tendency to distort economic decisions and to retard efficient economic growth? (Such a formula would also possibly be the fairest; but discussions of fairness are likely to be inconclusive.) Income is created by human and material resources. The resources utilized by a business as a whole in producing its income can be measured reasonably well by what is paid for them. Moreover, the places where the resources have been producing during a year can be determined on a consistent, though not completely unambiguous, basis. In numerous cases "separate accounting" for activities in each state is feasible and is now permitted. However, where it is not—and for many firms most affected by recent decisions separate accounting will not be possible—what other basis can serve?

As a starting point it seems acceptable to assume that property and labor contribute to net profit in proportion to what the firm pays for them. That is, more of the income of a business arises in the state in which payrolls are large

[3] There is, however, extensive variation in the factors utilized, their weighting, and in the definition and practical application of each element. For example, some states say that sales are located at the office where they are negotiated, others where the property is at the time of the order, or where the order is accepted, or where the negotiating personnel are located, or at point of delivery, or at origin of shipment—or at some other place. Several methods are also used in determining the situs of payrolls.

rather than small—and in direct proportion to the amounts.[4] The same applies to property. For example, one-eighth of the payroll and one-sixth of the property may be in one state. Then one-eighth of whatever payrolls (labor) contribute to net income, and one-sixth of the contribution of property, will be attributed to the state.

If this procedure is accepted, the problem then arises of determining how much is spent for labor and how much for the services of property—in total and in each state. Payroll data are available. However, figures for annual "spending" on services of property do not exist in any form that is usable for the purposes at hand. Some of the "payment" for the contribution of property takes the form of profit. And we certainly do not know the profit to allocate to each state—that is just what we seek to determine. Capital values are available, however, and these do reflect the values of the services of property.

Thus there is reason for the established practice of computing payrolls on the basis of annual outlays while using capital values for the property factor. However, if payrolls and property are computed on different bases, how can the results be combined? A simple weighting of the fractions seems acceptable. One possibility is to weight on the basis of national income aggregates. Payrolls would then carry about four times the weight given to property. Yet for the businesses involved—very largely manufacturing but also some finance and commerce—property can be more important than it is on the average. While a two-to-one weighting would tend to underweight the labor element somewhat, I am inclined to support it tentatively—and to invite comments and suggestions for more rational treatment.

C. Lowell Harriss*

[4] An economist might well insist that net profit is attributable to equity capital, other factors being paid as much as they are worth at the margin. I see no basis, however, for apportioning a firm's equity capital among states except on a basis no less arbitrary than involved in the more conventional proposal made here.

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Principles of Debt Management: Comment

In the June 1957 issue of this journal, Earl R. Rolph[1] undertook to show how the public debt should be managed to minimize the total interest charges while using the size and composition of the debt to achieve any given stabilization goal. Rolph states that "the composition and size of an outstanding national debt is optimal when the marginal utility of each kind of debt instrument is made proportional to its marginal cost" (p. 302). Without regard for whether the author is correct in considering it desirable to minimize interest costs, the purpose of this paper is to show that Rolph has made two analytical errors which may seriously damage his analysis.

He uses the term "moneyness," which all debts have in varying degrees,

[1] Earl R. Rolph, "Principles of Debt Management," *Am. Econ. Rev.*, June 1957, *42*, 302-20.

depending upon their liquidity and lack of risk. The shorter the maturity and the more these qualities are present, the greater the moneyness of the security. Gross national product is assumed to be a function of what I would call the total "money-equivalent" in the economy (which is the sum of all money and near-money in the private sector of the economy, weighted by the degree of moneyness of each). Gross national product may be reduced by removing money from the economy and substituting debt with less than 100 per cent moneyness. The less the moneyness of the debt, the more effective a deflationary tool it is.

A graphic analysis is used to determine the distribution of money and debt maturities needed to maintain a given gross national product (p. 312). Only debts of two maturities are assumed, one short-term and one long-term. Starting with any given distribution of long-term and short-term debt, if the government were to buy back an amount of long-term debt and sell an equal amount of short-term debt to the private sector, the amount of genuine money in the economy would be unchanged. However, a poorer substitute for money would have been replaced by a better one. This would be inflationary. To keep the total money-equivalent constant (and GNP as well), when a given amount of long-term debt is bought back, the total outstanding debt must be increased by selling a greater amount of short-term debt. Thus, as Rolph correctly states, an isoquant of gross national product may be represented by a line sloped negatively greater than 45 degrees. This would be a straight line if the substitutability of the two forms of debt for money were constant. But, as successive substitutions of short-term for long-term debt (into the private sector) are made, short-term debt becomes an increasingly poorer substitute for money as the total debt increases and the money supply decreases.

Having come this far without mishap, Rolph now becomes the victim of his own terminology. He reasons that since short-term debt becomes an increasingly poorer substitute for money when short-term debt is substituted for long-term debt, increasingly greater amounts of short-term debt sales are needed to counteract one unit of long-term debt bought (p. 313, n. 20). He therefore concludes that the curve will be convex to the origin. We started, however, with the premise that the poorer a substitute for money a debt was, the more deflationary its sale would be. Thus, Rolph is saying that the more potent a deflationary tool short-term debt becomes, the more of it is needed to achieve a given amount of deflation. Obviously, he is in error.

To put it another way, as short-term debt becomes an increasingly poorer substitute for money, it comes nearer and nearer to long-term debt in degree of moneyness. As short-term debt becomes more and more like long-term debt, the differential between the amounts of long-term debt bought and short-term debt sold must decrease. Therefore, as short-term debt is substituted for long-term, less and less of it is needed to counteract a given amount of long-term debt. The isoquant of gross national product will not be a curve convex to the origin, as Rolph has drawn it, but concave to the origin, with a negative slope (decreasing in steepness to a lower limit of 45 degrees, as the debt approaches 100 per cent short-term, if the limiting case is equality of moneyness).

A similar set of isoquants of total interest cost are also concave to the origin, as shown by Rolph (pp. 314-15). (At all points, it is assumed, long-term

debt has the higher yield, but increasing the size of an issue raises its yield.) Unfortunately, we now have two concave curves, instead of one concave and one convex. Now, we can no longer simply label the point of tangency bettween the desired GNP isoquant and any total-cost isoquant as a minimal cost distribution. With two sets of curves, both with substantially the same restraints upon them (concave to the origin, with a negative slope falling toward, but never quite reaching 45 degrees), the minimum cost point will be quite troublesome. It is highly possible that this point may lie on one axis or the other. It may also be highly unstable, and alternate between the two extremes. However, the importance of the altered graph lies not in the instability of the minimal cost point, but rather in the stability of the total interest cost as the distribuion changes along an isoquant of gross national product. Obviously, the more similar in shape the two sets of isoquants, the less effect changes in the distribution will have on the total interest cost.

Rolph next turns to the general case, where debts of all maturities are considered. In his Figure 4 (p. 317), with years to maturity on the X-axis and yield on the Y-axis, he graphs an interest rate (Y_m) rising with the length of maturity. A "variable multiplier" (not graphed) is, in effect, ($1 debt)/(effect of $1 debt) for each maturity. If graphed, it would be a falling function. From these two functions, he derives an "economic yield" curve (Y_e), which is Y_m multiplied by the variable multiplier. Y_e is, therefore, (interest rate) ($1 debt)/(effect of $1 debt). Thus, Y_e is the average cost of a given deflationary effect. According to Rolph, "Economical debt management calls ideally for making the Y_e curve a straight horizontal line. The interest on a national debt is a minimum when the interest cost of each type of debt instrument per dollar of 'product' (*i.e.*, change in private expenditures) is equal" (p. 317).

Rolph's solution for minimizing total interest cost makes the *average* cost of a given deflationary effect obtained from each kind of debt instrument equal, rather than making the "marginal utility of each kind of debt instrument . . . proportional to its marginal cost," as he has originally proposed. This is clearly not the same. The government may be viewed as a monopsonist buying various amounts of product (depressionary effect) from various markets (different maturities). Such a monopsonist would not minimize his total cost by distributing his purchases so that all prices were the same, unless the elasticities of supply were the same in each market. Therefore, there is no reason to presume that a horizontal Y_e curve minimizes the total interest cost.

The fact that this technique is faulty is relatively unimportant. The inherent similarity in shape of the isoquants of gross national product and total interest cost, discussed earlier in this paper, points to the unimportance of the debt distribution (along a GNP isoquant) as a determinant of the total interest cost. If this is the case, Rolph's analytical procedures, correct or incorrect, have but little economic significance.

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*The author, a graduate student at The Johns Hopkins University, would like to thank Professors Edwin S. Mills and Fritz Machlup of The Johns Hopkins University for their helpful criticism.

Principles of Debt Management: Reply

Friedman's first criticism concerns the case of a two-dimensional debt structure consisting of long-term and short-term securities. I held that the substitution of short-term for long-term debt in such a way as to leave private expenditures unchanged would require that the total debt increase, and that more and more short-term debt would be needed to offset a unit of long-term debt. Friedman asserts that the latter point is incorrect. Less and less short-term debt would be needed to offset a unit of long-term because the special attractiveness of short-term debt, what I called its moneyness features, diminishes as more of this debt becomes available. Friedman is clearly right in this observation. The GNP isoquant should have been depicted as concave rather than convex. Attention may be called to his demonstration of the precise character of such an isoquant in the limiting case. This finding is a positive contribution.

With two concave relations, a unique solution for a two-form debt structure is no longer assured. Corner solutions become possible; Chairman Martin's "bills-only" doctrine and Henry Simons' "perpetuities-only" doctrine may both be correct. Having made a good point, Friedman goes rather far in claiming (see his final paragraph) that the maturity distribution does not matter, at least not much. His conclusion holds if the relevant GNP and the total interest cost isoquants approximately coincide. How does Friedman know that they will? The interest expense of any actual two-form debt structure may exceed the minimum (or minima) by many times. Empirical questions cannot be resolved so easily.

Friedman's second criticism concerns the general case of a public debt structure of all maturities. He objects to my condition that debt-management officials should aim at making the weighted yields of different maturities (weighted by their comparative effects upon private expenditures) identical, holding that this condition is valid only in the event that what he describes as the elasticities of supply happen to be equal. I interpret this criticism to mean that if the demand schedules to hold government debt of different maturities are negatively sloped, officials should be guided by the weighted yields computed from marginal revenues rather than from market prices in selecting among debt forms to buy back or to sell. Hence, unless the price elasticities to hold different debt forms are the same, the government could save interest expense by concentrating the debt in forms having relatively high elasticities of demand. Such a rule calls for equating weighted yields of different maturities computed from marginal revenues rather than from prices.

As a formal proposition, such a rule would reduce the interest expense more than the one I proposed. But it is half-way solution. The interest expense could be reduced even more by segregating private demands to buy and sell debt and by charging and paying different prices to various classes of debt customers (Pigou's third-degree price discrimination). Conceivably even all-or-none deals could be made with each potential buyer or seller. Minimizing expense, like maximizing profits, is meaningful only with respect to defined constraints. Friedman's solution as I interpret it calls for treating market demand sched-

ules for debt as the constraints. My solution calls for treating market prices as the constraints. The debt structure and size are to be manipulated by reference to existing prices (and hence yields). The altered debt combination will result in changed yields. The process is to continue until yields, as computed from market prices and weighted by the relative effects of different maturities upon private expenditures, are made equal. Friedman's solution requires a similar experiment when the yields are computed from marginal revenues. Hence, the various elasticities of market demand schedules for different maturities must, somehow or other, be ascertained. Such information would be difficult if not impossible to acquire.

The original article was designed to suggest workable rules for the conduct of debt-management and open-market policies. As such, the proposals might be criticized as expecting too much, rather than too little, information and skill on the part of officials in charge. Friedman's solution calls for yet more knowledge. Both solutions are correct on theoretical grounds; others may also be correct, as indicated above.

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Erratum

Re the review of Eli Ginzberg's *Human Resources: The Wealth of a Nation* (this *Review*, March 1959, pp. 204-5), Eli Ginzberg is not Director of the National Manpower Council but is Director of Staff Studies for the Council. The book was written by him in his capacity as Director of the Conservation of Human Resources Project, Columbia University.

BOOK REVIEWS

General Economics; Methodology

Exacte economie. By D. B. J. Schouten. Leiden: H. E. Stenfert Kroese, 1957. Pp. viii, 226. *f* 18.–.

Professor Schouten, of the Catholic Economic Institute in Tilburg, Netherlands, has attempted a reformulation and synthesis of economic theory, including doctrinal history, micro- and macro-theory, growth and cycle theory, and international economics. The bulk of the book consists of ten models, described as: (1) "Quesnay à la Leontief," a system of intersectoral supply, in which labor is superabundant and land is the only scarce factor of production; (2) the growth theory of Ricardo, in which both labor and good land are superabundant and capital is the only limiting factor; (3) the stationary theory of Ricardo, in which good land is scarce and poor land and capital are superabundant; (4) the growth theory of Marx, in which capital is scarce because of excessively rapid technological progress; (5) the Walrasian system, in which substitutability among final products and the influence of prices on consumption patterns make possible the full employment of all factors of production, thus undermining the concepts of absolute scarcity or superabundance of particular factors; (6) the Keynesian system, in which a superabundance of capital entails underemployment equilibrium; (7) a business-cycle model in which lags and expectations generate fluctuations in the volume of production and employment; (8) a business-cycle model emphasizing price fluctuations, in which Tinbergen's relation between investment and profits plays a key role; (9) "the modern price and production theory," in which substitutability among factors of production limits the relevance of the concept of superabundance of a factor; and (10) the theory of international trade, in which freedom and restriction of commodity and factor movements are compared.

According to Schouten, his study is distinctive in using a uniform method and uniform algebraic symbols to bring order into the apparent multiplicity of economic theories and thus highlight their correspondences and divergences. Except in the two cycle models, Schouten adopts what he calls "the method of linear programming." This, however, goes little beyond graphs measuring consumption-goods production on one axis and investment-goods production on the other and representing by straight lines the output limitations posed by the available stock of each factor of production, given the technology. Each model is also cast into a set of equations expressing definitions, expenditure on consumption and investment as related to real income, technological relations between factor input and product output, supply-and-demand equilibrium for factors, and pricing. Finally, each model has an elaborate numerical example illustrating determination of the unknown quantities and prices. The mathematics is restricted to arithmetic and elementary algebra; though the

multiplicity of symbols, equipped with subscripts and superscripts, does present a formidable appearance. Only in appendixes does Schouten venture into the higher mathematics of inversion of the Leontief matrix and solution of dynamic equation systems.

The book amply illustrates the fact, incidentally, that use of symbols does not guarantee precision of thought. Schouten quite casually bandies about words or symbols for such things as "good" and "poor" land, "consumption" and "investment" goods, the minimum basket of consumption goods required for supporting a worker's family, the "total welfare" of two countries considered together—and "superabundant" and "scarce" factors of production. The latter distinction plays a peculiarly important role in squeezing the doctrines of earlier writers into Schouten's preconceived framework. His avowed attempt to organize a presentation of economic theory along the lines of doctrinal history results not only in lopsided theoretical emphasis but also in misrepresentation of the earlier writers. (For example, Ricardo is said to have considered only labor costs in his international-trade model because of a special assumption that not capital but only labor was a scarce factor.) Sometimes it is difficult to tell just when Schouten slides from ostensible paraphrase into commentary of his own. A complete absence throughout the book of references to any specific writings heightens the reader's, and perhaps betrays the author's, uneasiness on this score. Also missing are a bibliography and an index (and the table of contents is not detailed enough to serve as a substitute).

Not only Schouten's whole approach but also some specific comments (e.g., pp. 3-4) suggest that he regards various theories less as steps along (or sometimes astray from) the path of scientific progress than as schematizations of objective conditions supposedly prevailing or foreseeable during the lifetimes of the various economists. He deals with separate *models*, not with a continuing quest to find underlying uniformities in the superficial diversity of economic phenomena and to develop generalizations of broad explanatory and predictive value. His book conveys no appreciation of economics as an evolving, cumulative field of inquiry.

The purely negative impression that this review may so far have created is unintentional. The book is indeed a meritorious performance, demonstrating the author's creativeness and the breadth of his scholarly interests. This reviewer is unenthusiastic chiefly because Schouten's wide-ranging book is not tailored for any particular audience. It is neither for the beginning student nor for the sophisticated theorist. (Appendix 1, for example, contains a survey of standard marginalist theory which is too condensed and sketchy to be of use to either.) The book is not for the economist seeking an introduction to linear programming, not for the mathematician or econometrician, and certainly not for the student of doctrinal history. Some readers may find ideas useful for quasimathematical model-building. Students interested in this sort of thing might do better, however, to start with something like E. F. Beach's *Economic Models* (New York 1957).

Leland B. Yeager

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Common-Sense Economics. By G. M. Tucker. Harrisburg, Pa.: Stackpole Co., 1957. Pp. xiii, 289. $3.75.

When a creed is newly born its enthusiasm and intellectual drive often make it an admirable performance. Perpetuated beyond its time, the creed becomes less attractive. When two such creeds deriving from different social periods and reflecting different intellectual moods are consolidated with disharmonies ringing to the ear and the whole unsuited to the times, is anything more lost or futile or forlorn?

Substantially, this is the case with Tucker's book which effects a merger of the social statics of Herbert Spencer, restated in the idiom of an extreme "free-enterprise" credo, and the social philosophy and economic analysis of Henry George with his hostility to landed property and to rental incomes. The book is in form a textbook of economics with reference almost exclusively to American conditions.

Space in the book is allocated between a brief statement of the philosophy of work and wealth (pp. 1-65), an analysis of money and of inflation (pp. 65-95), a vigorous defense of Big Business (pp. 104-38), a short treatise on public finance (pp. 138-224); and social philosophy variously applied to current over-all trends (pp. 224-87).

The reviewer is not unsympathetic to the concerns which have driven the author. And the presentation in the book doubtless has a sparkle and persuasiveness which the conventional text could well emulate. But nonetheless the book is a dangerous one. The parade of dogmatisms does offend and the creed is set forth with little protective foliage of established fact or scholarly endeavor. It is simply not true that "practically all the world's monetary gold" was "buried in Kentucky" (p. 196). Wesley Clair Mitchell did not end some "four hundred pages of dreary reading" by "casting doubt" on the very existence of cycles, "implying that we are searching for something that does not exist" (p. 95). Tucker has confounded the punitive tax which drove out the circulating notes of state banks with the required "backing" of federal bonds stipulated as collateral for issuance of national bank notes (p. 78). Big Business for him cannot offend and "if occasionally a big concern does seem to get the lion's share of one particular market, this influence is counterbalanced a dozen times over by this greater and broader competition" (p. 130). J. M. Keynes is noticed as "a British economist of a kind" (p. 142).

The argument for the single tax raises some interesting issues. Tucker emphasizes that our present property taxation discourages new urban building particularly of rental property. This thesis in more sophisticated form has been recently developed by Morton in his *Housing Taxation* (1955). Transformation of the present property tax into a tax on urban land values would probably stimulate property renovation and improvement and this chiefly in older city-central areas where tax rates are heaviest and where deterioration cumulates. But Tucker does not recognize that this transformation would redistribute tax burdens among local residents so as to make the property tax less progressive in a general sense (or more regressive technically). The fact that business enterprise would contribute less by indirect levies to local government services is a possible advantage which Tucker over-

looked. But unless well-to-do families use frontage and acreage in proportion to their investment in improvements, then the concentration of urban levies on site values would redistribute levies among sites on more regressive lines. Tucker as a libertarian liberal bucks at progressive taxation generally. But most of us would hesitate at increasing regressivity in local taxation.

Over time, of course, a system of taxation of land values would encourage concentrated building on smaller lots. Besides, on impact the tax would virtually expropriate the equity of holders of present vacant lots which would probably revert to public ownership. Tucker continues to chant of land monopolists. He has not noticed that the bulk of our urban and rural land is broken up in small holdings; that urban land values have been greatly cheapened by the street car, bus and automobile; and that net rental incomes make up a small share of national income.

MANUEL GOTTLIEB

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The Functional Economy: The Bases of Economic Organization. By BERNARD W. DEMPSEY. Englewood Cliffs: Prentice-Hall, 1958. Pp. xi, 515. $6.00.

Father Dempsey's book is not an economic principles text in the common (or meaningful) use of that term. It is not a work in economics either, as that word is generally employed. *The Functional Economy* is an interpretive work on Catholic social philosophy with respect to the organization of all aspects of society. As a serious work it is subject to Father Dempsey's *apologia* on Catholic proposals for reform in general:

> Catholics sometimes believe that the indifference with which professional economists meet their proposals of reform stem from bias. In a very great number of cases this is certainly not so. What has happened is that the economist has been alienated by what he must inevitably consider sheer utopianism, since it is not accompanied by any reputable economic analysis (p. 330).

In most respects this book will be treated with the indifference noted above and in large part for the reason stated.

> *The Functional Economy* examines systematically the forces and facts present in every real, working economy. The author projects the ideal situation, which intelligent adherence to the enlightened teachings of the Church and constant awareness of the lessons of history can achieve, against the background of historic and existing economic communities. . . . The necessity of and the means for improvement of the economic community, the great challenge that lies in man's call for the restoration of social order, the historical development of man's moral attitude toward economics—all this has been analyzed in the light of traditional philosophy (p. v.).

Unfortunately, the text does not bear out the promise of the Preface. In Father Dempsey's system "All of the persons who make up any society have one supreme unconditional end, God" (p. 367). All have one earthly goal, the "perfection of human personality in this life" (p. 187, 191). All human society must be ordered to that goal, since man lives not isolated but in social groups.

Such perfection in human personality can only be achieved when that "social justice" has been established which requires a proper ordering of all aspects of life (p. 207). Social justice exists in a society when the relationships of man to man, organized community to man, and man to community are just; *i.e.* when commutative, distributive and contributive justice exist (p. 217ff., and Ch. 19). Social justice is the outcome of all human actions, and these in turn are subject to law.

> Human beings live under law, not merely the written law of the civil community, but the natural law of right reason, the divine eternal law, and the super-natural law of the kingdom of Christ. Law, the divine and eternal, governs all human actions, even those which civil law cannot reach, and all human actions, therefore, are related to the common good (p. 369).

The question neither raised nor answered in this legal connection is who or what on earth is the Court of last resort in the temporal, economic area?

In addition to the principle of social justice, "much more important . . . is the principle of subsidiarity" (p. 243). "All communities are subsidiary to . . . the concrete human persons who compose them" (p. 243). The principle of subsidiarity has as the role of government the regulating of the small and (to Father Dempsey) minor portion of man that is political. Such political government must be subsidiary to the groups (principle of functional association, p. 319) that form the good society. And the government's role in economic affairs should be limited to Adam Smith's point of view, for Lincoln's reasons (p. 282), based of course on Mill's "On Liberty"—which is not cited (see pp. 242, 495-96).

In all cases where the work might be considered "economic analysis" something is left to be desired (e.g., pp. 54-55, 99, 200, 426, 430, 439). And the scholastic form of argumentation that is at times employed makes, at best, for needlessly difficult reading.

> Briefly labor is a cause of the whole value of the product: (1) current labor alone, I deny—current and past, saved labor, I concede; (2) is the whole cause of the whole value, I deny—is the partial cause of the whole value, I concede (p. 196).

Another reading difficulty is presented by awkward terminology: e.g., monopolist, meaning any entrepreneur (p. 346); or development, meaning only innovations à la Schumpeter (p. 356). And, in a few cases sources which one would expect are not given (cf. "Summary of Flow-of-Funds Accounts for 1953," a table, p. 50).

In spite of its shortcomings this work serves two interests well: those interested in the Thomastic-Holy See economic philosophy (not analysis), and those new non-Catholic Ph.D.'s interested in employment on a Catholic faculty. I would suggest, also, that those who cite from papal encyclicals and from Aquinas in their courses would make use of Father Dempsey's work.

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Selections in Economics. Edited by R. C. Epstein and A. D. Butler. 2 volumes. Buffalo: Smith, Keynes and Marshall, 1958. Pp. viii, 247; viii, 216. $4.50; paper, $2.95, each.

These two volumes combined contain 25 selections, averaging about 6,000 words in length. Each selection is either a complete article or a complete chapter from a longer work. Most of the authors are distinguished modern economists.

The editors claim two purposes for their selections; to supplement elementary texts and to interest readers ". . . who seek to bring their earlier and more formal study of the subject up to date." The second purpose is not realized. The first volume does provide an up-to-date treatment of certain phases of macroeconomics, but the most important selections are highly formal. It is doubtful that they could be read with comprehension by anyone who is the least bit rusty in economics. As far as the treatment of microeconomics in the second volume is concerned, there would be nothing new for a student who had not looked at a book in the area since 1930. This section is not only out-dated; it is positively old-fashioned. The four theoretical selections in value and distribution theory all depend on utility analysis and the assumption of pure competition. For these reasons, I will consider the value of the selections for class-room use rather than for the general reader.

It seems apparent that the typical readings book in economics is designed for a "survey" or "problems" course rather than for a "principles" course. The brevity and disputatiousness of the bulk of the selections in most readings books seem to be intended to lend artificial life to an intrinsically boring subject. As the teacher of a real principles of economics course refuses to admit that his subject is inherently dull, this type of book is of little value to him. The ideal outside readings for such a course should clarify or illustrate theoretical points that are difficult for the beginning student, especially when these points are not well developed in the text. It should not set forth debatable conclusions without premises or proof, but should provide a full logical development of each topic treated.

It is apparently the intention of Professors Epstein and Butler to provide just such an ideal supplement for use in the principles course. They have succeeded to a substantial extent in their first volume, which covers income theory, money and economic fluctuations. The selections in the second volume, covering value and distribution, labor, international trade and economic development, are less satisfying. I have been unable to call to mind any widely used principles text that would be substantially strengthened in its coverage of value and distribution by these readings, although this is the weak point of many currently popular texts.

The selections vary widely in difficulty. While one third of them are easy selections from principles texts, another third make no concessions whatever to the beginning reader. Thus the essay by Keynes, which the editors feel to be ". . . far clearer than his exposition of some of the same ideas [in *The General Theory*]" is likely to be quite obscure to American students even after completion of the principles course, for it assumes a better understanding of the internal effects of international balances than most of our students

acquire. Similarly, some of the other selections require a more penetrating understanding of several areas of economic analysis than can be hoped for at any stage of the principles course.

In reproducing the chapter on "Supply and Demand" from Bye's *Principles of Economics* the editors have given new life to old errors in the treatment of decreasing supply price. Mitchell's article on business cycles contains a less important error in his discussion of the acceleration principle, stemming from an implicit assumption that disinvestment is impossible. There are also a number of debatable conclusions and half-truths that have no place in an authoritative work, though many of these are in the popular articles.

The high price of the two volumes, together with the weaknesses mentioned above, limit the value of these selections for use in the principles course. However, the first volume may find wide use in intermediate level courses in macroeconomics or money and banking. One may hope that its sale will be large enough to bring forth a rash of cheap imitations.

V. F. Boland

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Videnskab og veljaerd i økonomisk politik. (Science and Welfare in Economic Policy.) By F. Zeuthen. Københabns Universitets Økonomiske Institut, Stud. No. 1. Copenhagen: G. E. C. Gads, 1958. Pp. 97.

This essay inaugurates a new series of studies to be published by the Institute of Economics of the University of Copenhagen. Professor Zeuthen, the founder of this distinguished school, has for a long time been concerned with problems of practical social policy as well as with the advancement of economic theory. This book is an attempt to bridge the mental gap that usually separates these two fields of investigation.

The central problem is that of applying scientific method in a field so far dominated by subjectivity, dogmatism and ideologies. Is it possible to rationalize policy by applying experimental and logical methods? The many aspects of this problem are thoroughly, though concisely, discussed. Old and new welfare theories, starting with Bentham, are re-examined. The Paretian stumbling block of the dichotomy between the scientifically valid efficiency theory and the nonscientific distribution doctrines is once more discussed, and overcome by referring to common sense, which legitimates interpersonal comparisons of utility. Production and exchange on the one hand, and distribution on the other, are dealt with separately, before being merged again in the discussion of practical problems in the last chapter. Combining efficiency and distribution criteria in solving policy problems involves compromises whenever the two sets of criteria are incompatible. Distribution policy should be designed in a way to promote efficiency or, at least, to minimize any loss of production; conversely, a policy aimed at increasing productivity should have regard for the distribution effects. The problem of centralization versus decentralization in policy-making is discussed and solved in a nondogmatic way. The author

then discusses R. Frisch's and Tinbergen's attempts to rationalize policy-making by using decision models.

The author concludes, à la Robertson, by pleading for the use of common sense in addition to science in dealing with practical problems. To be useful for policy, science must build on plausible, not on arbitrary, assumptions. On the other hand, to be successful in the long run, policy decisions must be founded on sound analysis.

Although this book makes no new contribution to welfare theory, nor to the solution of concrete problems, it contributes mature wisdom, which is a comparatively rare service.

ROGER DEHEM

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Investment in Innovation. By C. F. CARTER and B. R. WILLIAMS. New York and London: Oxford University Press, 1958. Pp. ix, 164. 15s.

This book, a sequel to *Industry and Technical Progress* by the same authors, is sponsored by the Science and Industry Committee appointed by the Royal Society of Arts, the British Association for the Advancement of Science, and the Nuffield Foundation. Although it draws its facts from a study of British industry, its conclusions ring true for the United States as well.

The Appendix at the end of the book might well be read before the first chapter, because this appendix outlines the orthodox theory which the book seeks to improve. Here is set forth the classical theory on saving and innovation as worked out by Adam Smith, Ricardo, Mill, Marshall, Pigou, Knight, and others. A very interesting account it is.

New inventions, new products, new methods, new scientific knowledge, all these things give entrepreneurs an opportunity to invest in new capital goods with the hope of high profits. This book would have been of great interest, therefore, to J. B. Clark, with his theory that business profits, the fourth distributive share, are the reward for successful pioneering. It would also have been of interest to Schumpeter, with his theory that the exploitation of great inventions one after another is what causes business cycles. It would likewise have been of interest to Keynes, with his theory that planned investment may differ from planned saving; and also of interest to Hansen with his theory of secular stagnation. And today the book is of especial interest to the Board of Trade, who financed its publication, because the Board wants to know what if anything can wisely be done by the government to control the business cycle.

The book asks questions like these: Where do new ideas come from? What sort of ideas are most wanted? How are they sifted? Who makes the choice in the end? How is allowance made for risk and uncertainty? Is there any shortage of ideas? Is there any slowness in making use of them? Is there any shortage of money to exploit them? What rate of return is needed to get an innovation accepted? Do corporations with great research laboratories (like Bell Telephone, General Electric, and du Pont in the United States) have a customary cut-off point between acceptance and rejection? Does this cut-off point change with changes in the rate of interest?

In answer to the last question, the book cites a long list of case studies to prove that the rate of interest is usually of small importance compared with the outlook for profits, the volume of unfilled orders, the firmness of prices, the burden of taxes, and the amount of excess capacity in the industry.

Would it have been digressing too far from the main theme if the authors had gone on to show that certain other kinds of investment—in contrast to investment in innovation—are indeed very sensitive to changes in the rate of interest? They could have mentioned investment in private housing financed by home mortgages, and they could have listed public works like schools, streets, sewers, waterworks, bridges, subways, etc., financed by municipal bonds that boost the tax rate on real estate, and they could even have included long-term Treasury issues used to refund the floating debt, for in all these cases the flow of new issues is very sensitive to the current price of bonds and the success of recent offerings.

The authors, by asking the right questions and going to business managers themselves for the true answers, deserve our hearty approval. Their book is a real success, because it adds a great deal to our understanding of the free enterprise system and how it actually works.

John Burr Williams

Wellesley Hills, Massachusetts

Studies in the Mathematical Theory of Inventory and Production. By Kenneth J. Arrow, Samuel Karlin, and Herbert Scarf. Stanford: Stanford University Press, 1958. Pp. ix, 340. $8.75.

The authors of this book are to be commended for carrying out a highly competent and rigorous analysis of certain inventory and production problems. The mathematical level of the book is such that the vast majority of the readers of this journal will find it difficult if not impossible to follow.

The problems analyzed are quite general. The authors search for policies which minimize the sum of certain costs associated with various levels of production and inventory when confronted by stochastic or deterministic demand. Cost minimization is equivalent to profit maximization, since prices are considered as fixed and not controlled by the decision-maker. Among the costs considered are those of producing or ordering, costs of storage, and costs of stock-outs. The solution of these problems is of considerable practical as well as theoretical interest.

Unfortunately, from the standpoint of the economist, the authors have made little effort to relate their results to economic theory, except for a brief portion in the introductory chapter drawing the parallel between inventory theory and the transactions, precautionary, and speculative motives. The relevance of the authors' analyses for microeconomic theory receives extremely little attention. Although this is not the purpose of the book, the economist cannot help but be disappointed at the lack of concern with problems of interest to him. The pure mathematician also finds little of great interest to him. However, applied mathematicians, mathematical economists, and operations researchers should find the book interesting and relevant. For the nonmathematical reader, the second chapter provides a description of the nature of the problems considered and the techniques used to solve them.

Part II analyzes optimal production and inventory policies in deterministic processes. It deals with problems involving production over time with increasing marginal cost, production smoothing and optimal production plans without storage. Some of the equations contain familiar results such as the difference between the marginal costs in two periods equaling the cost of carrying stocks from one period to the other (p. 63). However, many of the cases contain phenomena usually not considered in traditional economic literature, such as the requirement that initial inventory plus cumulative production must be at least equal to cumulative sales at any point in time.

Part III is concerned with optimal policies in stochastic inventory processes, demand being the primary stochastic variable. The following example may serve to give some of the flavor of the analyses. Assume that one wishes to determine the optimal stock level for a single period when demand is stochastic. A literary economist might suspect that the optimal policy would involve equating expected marginal cost and expected marginal revenue. In most cases, his suspicions would be confirmed. However, mathematical analysis includes, in addition, various pathological cases (e.g., among others, a zero or infinite optimal stock) which do not lead to an unique solution, bringing to light the hidden assumptions often implicitly made by the economist. It is to be hoped that the same rigorous microscopic analysis may be extended to other areas in economic theory, although such rigor is necessarily achieved only at the expense of much tedious detail.

The final portions of the book are concerned with operating characteristics of inventory policies, expecially policies of "simple form," which are optimal under many plausible sets of circumstances. Their conclusions are of interest from the point of view of business applications, although even some of these "simple" rules are presented in a more complicated form than most that have reached the application stage.

The book ties together many loose ends of analysis in inventory and production theory. It is recommended reading for those with a good mathematical background who are interested in problems in this area.

T. M. Whitin

Massachusetts Institute of Technology

Inventories and the Business Cycle with Special Reference to Canada. By Clarence L. Barber. Toronto: University of Toronto Press, 1958. Pp. xii, 132. $3.50.

From the time of the Classical School's pronouncements concerning the relationship between circulating capital and economic crises, economic analysis has made great strides; but the activities about which economists theorize have increased greatly in number and complexity—possibly as much or more than our advances in theoretical understanding of what goes by the name of business cycle. Inventories have long been suspected of being one of the crucial elements in the problem of cycles, so we are fortunate in having another contribution towards an understanding of their rôle in economic fluctuations.

The present volume originated, as have many notable works, in a doctoral dissertation. Professor Barber starts with a doctoral candidate's summary of the literature and of the diverse findings in his special field. He

then goes on, in keeping with current methodological procedures, to develop a model relating investment in inventories to equilibrium levels of income. The model is based, essentially, on Keynes' famous formulation of the relationship of income to consumption, investment and saving. This is the major theme in Part I, but even in this part Barber's discussion breaks away at times from a strict preoccupation with his models, and he gives some penetrating comments on the problem of inventories as seen from empirical evidence.

In Part II, on "Inventory Fluctuations in Canada, 1918 to 1950," the author describes certain industries and their inventory experience, and then he examines the entire economy and inventory data. He describes different forms of inventory and differences in major sectors of the economy such as manufacturing and agriculture. He brings out the possible divergence between the value and the volume of inventories, showing how different accounting formulæ affect inventory values and profits. He shows that inventories are only one form of investment and gives its relative significance in the Canadian economy. He mentions the inventory investment-bank loan relationship; and he indirectly raises the issue whether an increase in inventories may not be the effect as well as a cause of the change in the level of economic activity. He also shows some interesting differences between the Canadian and the United States economy in cyclical behavior and in inventory policy. The latter seems a wonderful field into which to probe further.

The number and intricacy of the factors impinging on inventories, which the author clearly brings out, show that much more spade work will have to be done in this field; and, to the reviewer at least, indicate that it may be a little premature to spend too much time building macroeconomic models for inventory-business-cycle analysis. We still have a lot of agonizing work to do in formulating the questions we want answered and in bringing some order out of the complex diversity of phenemona. Barber's study will be an aid to others and should lead to further work.

RICHARD C. BERNHARD

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Risparmio e ciclo economico. By GIANCARLO MAZZOCCHI. Milan: A. Giuffrè, 1957. Pp. 168. L. 1200.

This volume on savings and business fluctuations is concerned with the interesting problem of the way in which cyclical movements may be modified by one form of personal savings, the contractual. This category which appears to be showing secular growth, as Goldsmith has shown in his monumental *Study of Savings in the United States,* is regarded by the author as the sum of life insurance premiums, contributions under private pension plans, and mortgage payments. (Instalment payments for consumer durables, which now commonly run as long as three years, are excluded.)

Mazzocchi's analysis, based on data and technical literature which are largely American and British, leads him to the conclusion that substantial contractual savings, coupled with the investment practices of the institutions which receive such savings, increase the economy's instability. On the savings side, an increase in the ratio of contractual to total savings reduces the marginal propensity to save which, given the multiplier, tends to increase

the amplitude and duration of fluctuations. Such fluctuations are not moderated but actually intensified by the investment practices of what we call financial intermediaries, which favor or are required by law to place their funds in debt instruments. Capital users wishing to maintain a given equity-debt ratio are guided in their use of debt capital mainly by the level of retained earnings. Because the latter are themselves markedly subject to fluctuations, however, savings of the contractual sort move irregularly into investment.

Suggested remedies are chiefiy of the fiscal kind. The author does not count on relief by way of monetary policy, because saving is held to be too interest-inelastic. Instead Mazzocchi seems to lean heavily on periodic recourse to a spending tax, under which the authorities could encourage saving during expansions and spending in recessions. On the investment side, he would like to see enlarged facilities for supplying risk capital, particularly to medium and small businesses, in order mainly to minimize the role of unstable retained earnings in connection with investment plans.

Though Mazzocchi presents a closely reasoned and lucid exposition of the subject, which is only sketchily treated above, this reviewer feels that his analytical account is one of a long list of efforts which needs to be rethought in the light of Friedman's basic critique of the consumption function.[1] Contractual savings would appear to take on a different significance, for example, if they were viewed as deriving in some measure from transitory components of income rather than from permanent income. Policy prescription, particularly that of the compensatory variety, seems premature in the light of the unresolved analytical questions.

VIRGIL SALERA

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Vpliv dohodkov in cen na raven potrošnje prebivalstva v Sloveniji. (The Influence of Incomes and Prices upon the Level of Consumption in Slovenia.) By VLADO FRANKOVIČ, ALENKA RISMAL, and VIDOJKA KOZAK. Ljubljana: Economic Institute of Slovenia, 1958. Pp. 140.

In October 1957, the Yugoslav People's Assembly adopted the resolution on Perspective Development of Personal Consumption. It recognized the adverse effect of a low level of personal consumption upon the productivity of labor. The resolution called for an increase in the share of national income devoted to personal consumption, and for development of more accurate measurements of fluctuations in real wages and personal consumption. The present book is a contribution to the accomplishment of the latter task.

The study is mainly concerned with: (1) income and price elasticity of demand for main foods as well as for broad categories of personal consumption (food, housing, etc.); (2) the structure of consumption at different levels of income; and (3) minimum cost of food necessary to satisfy qualitative and quantitative physiological requirements.

The analysis is based upon budgets of four-member families in Slovenia, with an income range of 15,000-30,000 dinars. Workers', clerical, and

[1] Milton Friedman, *A Theory of the Consumption Function*, Nat. Bur. Econ. Research Gen. Ser. 63, Princeton 1957; and the published comments thereon, especially Robert Eisner, "The Permanent Income Hypothesis: Comment," this *Review*, December 1958, *48*, 972-90.

agricultural families are treated separately. In determining the combined effect of prices and incomes upon the consumption, the function:

$$(1) \qquad p = \frac{a}{c} + bd \quad (p = \text{consumption},\ c = \text{prices},\ d = \text{incomes})$$

was selected. Income and price coefficients of demand elasticity for 1955 and 1956 were determined by keeping prices and incomes constant. The effect of the prices of substitutes (Slutsky-Schultz theorem) was not considered because of the complexity of mathematical operations, for the authors believe that the results obtained would not have been more significant, considering given economic conditions.

The coefficients of income and price elasticity for various foods are less than unity in most cases. The outlays for food are influenced more strongly by income than by prices. Outlays for clothing are characterized by low income and price coefficients because of relatively low consumption and high prices. The relevance of coefficients for housing is obscured because of the acute shortage of housing and because of noneconomic rents.

The structural analysis reveals a relatively low level of consumption, reflected mainly in a high percentage of food expenditures, which in Yugoslavia take 54.9 and 52.5 per cent of total expenditures (for workers' and clerical families respectively in 1956); in Austria, 49 per cent (for all population in 1954-55); in Switzerland, 38.2 per cent (for all population in 1955); in the United States, 28.8 per cent (for urban population in 1950); in Norway, 27.1 per cent (for all population in 1952-53).

The final two chapters deal with the analysis of the minimal cost of food, considering the physiological requirements. They present the initial results of research, still in progress, conducted by the authors at the Economic Institute of Slovenia. First the characteristics of a typical family were determined on basis of data obtained in the survey; then the daily physiological requirements of this family were found from the tables of the Food and Nutrition Board (U.S.A.), which were adapted to conditions in Slovenia. The basic problem is presented in the question: What type and quantity of food should the four-member family have bought per day, in 1956, in order to satisfy its physiological needs with a minimum of expense? The problem was solved by linear programming, the result giving a rather unvaried menu. To approximate habitual consumption, additional conditions, permitting the inclusion of other foods, were added in two subsequent programs. These additions, of course, increased the cost. Comparison of results obtained in the third linear program with the actual food consumption of surveyed families showed that the existing diet provided sufficient calories, but that it was unsatisfactory from a nutritional standpoint, particularly in the case of the low-income families. A further comparison of food expenditures of total population with the physiologically required minimum showed that actual expenditures will not purchase this minimum.

The study is a valuable contribution to the literature in the not widely explored field of Yugoslav consumption.

TOUSSAINT HOČEVAR

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Ricardian Economics—A Historical Study. By MARK BLAUG. Yale Studies in Economics, No. 8. New Haven: Yale University Press, 1958. Pp. x, 269. $5.00.

Ricardo was one of the most amiable and obstinate of economists who ever lived. He could hear disagreement with the bland indifference with which saints listen to heretics, and he could receive praise in the way they are said to listen to hymns. Neither meant much to him. He had a set of ideas, and it was the product of his formidable powers of analysis and observation. Mr. Blaug has written a good book about it, and about Ricardo's followers, their opponents, and the ways the system made itself felt. He has stayed close to the texts, and his method is sympathetic and searching: What were the economists trying to do, and how were particular ideas related to their intention?

Ricardo's was to explain the laws regulating the distribution of income. He meant, Blaug explains, the causes of *changes* in distribution as population increases and the productivity of land declines. More labor time is needed in order to produce a given amount of food. Real wages may fall in the short run, but in time will rise to the subsistence amount. On marginal land, the rate of profit diminishes, and the rate elsewhere must fall also. Rent, the difference between the outputs of equal inputs of labor-and-capital, increases. Over the long period, income is redistributed in favor of landlords, real wages are constant, and the rate of profit declines. "The heart of the Ricardian system consists of the proposition that the yield of wheat per acre of land governs the general rate of return on invested capital as well as the secular changes in the distributive shares."

It was the declining rate of profit which would cause the distribution of income to change. A complete explanation required an accurate statement of the labor theory of value and an invariable measure of value. Few who read Ricardo saw the place of those ideas in his system. They enabled him to show that additional investment in agriculture altered the terms of exchange between it and other industry in favor of the landlords. When M'Culloch (who cuts a better figure here than in most histories) proclaimed the harmony of self-interest, Ricardo demurred. "Are the interests of the landlords and those of the public always the same?" he asked.

If one accepts Blaug's statement of Ricardo's intention (as the reviewer does), one must conclude that there has been much said about Ricardo which is inconsequential. He did side with Say and believe that full employment was automatic, but the point was extraneous, and there is no excuse for feeling superior or becoming indignant about the "tyranny" of his system. There have been growth models put together out of elements culled from the *Principles.* They are quite synthetic, and impute a purpose to Ricardo which would have bewildered him. He would have been surprised even by his reputation as a champion of *laisser faire,* because policy was not his guiding purpose.

In that area, Blaug is not at his best. He is reluctant to accept the fact that Ricardo did not believe in free trade, although he describes Ricardo's position very exactly; and he attaches less importance than it deserves to the fact that Ricardo's followers opposed the campaign to repeal the corn laws. The normative ideas of the Ricardians are not as easily integrated as their positive ideas are. About Blaug's treatment of the latter, there is little with which

one can disagree. There are a few indefensible assertions, but they can be set down to haste or polemical zeal.

His book is a fair statement of the system in its positive aspects. Now let us have equally good statements of the others. We need a biography of Ricardo, and if an economist will not do it, let someone like M. St. J. Packe use the material. He told us many things about Mill which were illuminating (*The Life of John Stuart Mill*). Studies of the economists as political figures will help us to understand their ideas about policy. We also need fresh viewpoints like that of Walter C. Weisskopf, in *The Psychology of Economics*, which analyzes the premises of the classicists as well as the reasoning from them. Then let someone put his mind to a vocabulary of economics (a Fraser for the present) as T. D. Weldon has done for politics. Blaug's book will have an important place in such a collection.

William D. Grampp

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Theory and History, An Interpretation of Social and Economic Evolution. By Ludwig von Mises. New Haven: Yale University Press, 1957. Pp. ix, 384. $6.00.

With the author's indulgence, an advance estimate of the general professional reception to this work would indeed be low. In the case of many within the economic fraternity a quick, instinctive association between the author's name and his extremist position in the spectrum of contemporary economic thought may be sufficient to cause indifference. The limited interest of professional economists in the subject under consideration would also support a low estimate. However, despite these and other possible factors, a largely negative reaction would be most unfortunate. Obviously, a lack of serious interest in this treatise could not advance and deepen our thinking on some of the most fundamental intellectual issues confronting American minds today.

As a sequel to his tome on *Human Action*, in this absorbing study von Mises attempts to grapple with the fundamental issues of guiding thought. Looked at in its entirety, the work is not merely an intepretation of social and economic evolution; it represents von Mises' personal philosophy based upon years of study, scholarship, and cultured experience. Whatever may be said of his economic "classicism," in contrast to some others of like mind he brings into his expositions a wealth of cultural and historical insights. Although he will not agree with the reviewer's terminology, in the present study he displays dimensions of thought and embraces a broad orientation reflecting the stature of a social scientist with a formal economic background. Von Mises' lucid and often incisive contributions cannot but be readily welcomed. They serve both as a sobering counterweight to many perpetuated socio-economic fallacies and a vibrant stimulus for economists to explore more intensively related disciplines of thought.

The range and nature of subject matter covered in this work make it essentially a philosophical treatise rather than, as the author to some extent believes, a strictly scientific contribution. In examining, for instance, the differences between natural and social scientific methods, the question of ends and means, or the relationship between materialism and determinism,

the author is thinking philosophically, not scientifically. He subtilizes fairly well many such conceptions. But when it comes to a firm and grounded distinction between science and philosophy, none can be found; and confusion on this point persists throughout the entire work. To speak in one place of the "discursive reasoning of the sciences of human action" (p. 4) and, in another, of philosophical interpretations as being "discursive and scientific" (p. 323) indicates a loose handling of terms. When he speaks of philosophy building on "the foundations laid by science" (p. 275), he clearly shows a defective understanding of the independent discipline of philosophy. Science serves philosophy, to be sure, but so does the ever-expanding reservoir of common experience. The linearity and sensory observations of scientific thought as against the inherent discursiveness of philosophical thought and its intellectual penetration into the intelligible nature of things seem to elude the author.

This inability or perhaps reluctance to sharply define the two disciplines is crucial to a critical analysis of von Mises' dominant thesis and his supposedly novel introduction of the two terms "thymology" and "praxeology." His thesis is really a simple one. Indeed, it essentially and boldly projects into the full context of historical process what this reviewer in his work on *Veblenism* has defined as the formal object of economic science. Our chief concern, von Mises contends, is with the choosing of means suitable for the realization of man's ultimate ends. These ends depend entirely on judgments of value which are subjective, voluntaristic, and scientifically indeterminable. We are told that choosing means is a "matter of reason" and choosing ends "a matter of soul and will." Repeated in other words time and time again, the further point is made that: "With almost negligible exceptions, all people want to preserve their lives and health and improve the material conditions of their existence" (p. 270). Capping this philosophical scheme is the individualist utilitarian view that what results in benefits is good and what doesn't is bad. Applying the formula to social systems, one is to conclude then that von Mises' position on pure capitalism, i.e. without governmental intervention, is the true one because of the beneficial results produced by that type of system.

The simplistic character of this philosophical scheme is as unsatisfactory today as it was in John Stuart Mill's day. The existential ends of man include also self-preservation in terms of personal honor, self-perfection, enlargement of experience, and social fellowship to promote common utility. Doubtless, suitable means are important to realize ultimate ends, but a rational knowledge of good ends is equally important for the judicious determination of suitable means. And the good in general is the perfection that is proper to a thing. Such knowledge unavoidably necessitates the systematic study of metaphysics. It helps von Mises little to assert that man cannot do without metaphysics and then, quite erroneously, claim that it has no significance for "a logical examination of scientific problems" which, in this work, are almost entirely philosophical in character. The nature of man, which is common to all men, lends itself as an object of rational inquiry just as any other being in existence. Mere reiteration of subjective "feelings, tastes or preferences" by the author doesn't brush this truth aside.

The author's defense of the individuality of man, the primacy of ideas and reason, and the basic freedoms is impressively stated. His criticisms against positivism, historicism, behaviorism, and panphysicalism are logically sound. Notions associated with these movements have afflicted the social sciences, including economics. But the treatment offered by von Mises is no more formidable than were the unsuccessful attempts of the nineteenth-century liberals to stem the tide of scientific materialism. Were he familiar with the full structure of Western thought, whence these ideas on man, reason, and freedom flowed historically, his arguments would be strengthened considerably by more basic ideas rooted in metaphysically perceived reality.

On this point the book shows a gross unfamiliarity with the *Philosophia Perennis* formed in the medieval period. Inaccurate statements on the origin of the idea of the common weal (p. 30), the relationship between collectivist philosophy and medieval realism (p. 250), and the beginning of a stress on the problem of liberty and bondage by seventeenth century philosophers amply attest to von Mises' blind spots. There are many more, some of an exaggerated and wild nature. What he calls thymology has formed the content of philosophical psychology long ago; what he calls praxeology has also for centuries formed the empirical basis of social ethics. New terms do not solve essentially old problems. And socialism, for which, significantly, the utilitarian doctrine has been ably used, is not the only possible outcome of our efforts to preserve the good of the market economy and to reconstruct our institutional arrangements on the basis of durable principles of socio-economic order.

Lev E. Dobriansky

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Historia rozwoju ekonomiki. (A History of the Development of Economics.) By Edward Taylor. Poznan: Panstwowe Wydawnictwo Naukowe, 1957/58. 2 vols. Pp. xii, 258; 385.

The intellectual ferment which accompanied the October 1956 events in Poland was heavy with promise of valid contributions to knowledge, not least in the field of economics. The results to date have been disappointing. A striking exception to the run of mediocre economic journalism is Professor Edward Taylor's two-volume textbook on the *History of Economics,* of which the first volume appeared in October 1957, and the second in February 1958. A third volume devoted to the development of socialism, has been promised. The first two volumes are an eloquent tribute to the tradition of the University of Poznan as a center of Polish economic thought, and to the scholarship and academic integrity of the author. Here is a clear, incisive, well-balanced and comprehensive manual worthy of the attention of students of the subject, and crying out for translation into at least one of the world languages.

Taylor held the chair of economics at the University of Poznan from 1919 till 1949. In that year he fell victim to the purge of the allegedly "reactionary" instructors carried out by the communist government, and was not reinstated until the fall of 1956. Prior to his compulsory retirement and at the request of his students, Taylor in collaboration with one of his assistants, J. Tetzlaw,

prepared a revised script of his lectures in the history of economic thought, containing the substance of thirty years of scholarship and pedagogical experience. His retirement from active teaching in the years 1950-1956 provided him with occasion to thoroughly re-examine, expand, and rewrite the original lectures in book form. Although it bears the marks of its lecture parentage, the result is a refreshingly clear account of the development of economic thought from ancient times to the present day. This is not just another textbook on the subject; it ranks among the best manuals on the subject currently available on the international market.

The peculiar circumstances of their inception account for a number of the unusual features of the first two volumes. The "economic underworld," all that ballast of ideologically tinged socio-economic movements, is ruthlessly eliminated from this portion of the book. The removal of the untouchables of Marxist economics (and, by extension, of the whole host of utopian socialists and early collectivists) seems to have been a prerequisite for an objective treatment of the bulk of economic theory. This method has a surprisingly purifying effect on the exposition of the logical structure, evolutionary process, and continuity of theoretical economics. The voices-off-stage, significant as they may be, have been put on a separate record. The reader's attention is in this way concentrated on the world of ideas, on the disinterested pursuit of an explanation of economic phenomena, with only slight distraction from the historical facts of the epoch in which these ideas originated. Whether such procedure is to be considered as the book's weakness or strength is irrelevant in view of the circumstances under which it had been written and published. One example of the method may be of interest: born in the midst of a communist world, the book refers to communism once in all its 643 pages, and this only in connection with the ideological stand of Professor Oscar Lange.

Taylor attempts, successfully, to combine the two methods of presenting the history of economic ideas: he both outlines the logical development of particular economic problems, and presents a coherent chronological arrangement by authors. Major stress is laid on the post-1870 period. This is, without doubt, the most interesting and thorough part of the work, reflecting a great effort to convey clearly and concisely, yet without sacrifice of the essentials, the original contributions to the science of the last eight decades. As a contribution to the understanding of the issues involved, Taylor's exposition of the economic thinking of this period stands well nigh unique.

JAN S. PRYBYLA

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Ekonomi och religion. By KURT SAMUELSSON. Stockholm: KF's Bokförlag, 1957. Pp. 175. Sw. Kr. 11.

This essay is a vigorous attack on Max Weber and the propositions first advanced in *The Protestant Ethic and the Spirit of Capitalism.* It argues, first, that Protestant thought had little or nothing to do with the "spirit of capitalism" and, second, that there was no significant correlation between religious changes and the post-Reformation development of capitalism.

After an interesting recapitulation of various contributions to the half

century of debate over the Weber thesis, Samuelsson examines the attitudes of the reformers toward wealth and economic activity. He concludes that puritanism, far from being the source of the capitalist spirit, was antithetic to it. Most of the arguments of this section have been advanced by earlier critics of Weber and nearly all of them were either anticipated by Weber or answered in the notes to the 1930 English edition of *The Protestant Ethic.*

Samuelsson does not seem to understand the distinction, clearly formulated by Weber, between the immediate intentions of the reformers and the ultimate consequences of their ideas. His citations of the sermons of Fox and Wesley on the dangers of wealth miss the point that such sermons became necessary when relatively poor men had become rich in the practice of their religion. No evidence supports his contention that the reformers trimmed their doctrine to suit capitalist audiences or that businessmen changed their religion to fit their economic practice. Such allegations suggest an inadequate acquaintance with early Protestant history and imply an underlying conviction that religious attitudes could not have had much influence on economic behavior.

Previous discussions of the Weber thesis have, according to Samuelsson, been beside the point because they failed to ask whether there was in fact any correlation between Protestantism and economic progress. He embarks on a comprehensive historical review to demonstrate that the answer is negative, but his country-by-country analysis brings forth nothing new, and his interpretations are not impressive. England and Holland are shown to have experienced economic progress before the Reformation, but there is no discussion of varying rates of economic progress. The most prosperous periods in Switzerland and Scotland appear to have come after the decline of Calvinist influence, but this would be consistent with Weber's conception of the role of puritanism. Only a naïve view of historical relationships could accept the conclusion that such "discoveries" have undermined the foundation of the Weber thesis.

Samuelsson seems to believe that the idea of a relationship between piety and progress originated with Weber rather than in the comments of contemporary observers and subsequent scholars. His attempt to demolish the "starting point" of the Weber thesis is unconvincing, although he does unearth a typographical error in the Weber-Offenbacher statistics of Protestants in institutions of higher education in Baden.

In view of the sweeping nature of Samuelsson's charges, it is appropriate to recall the limited and precise aim of Weber's original essays. He sought to explain how an attitude (the spirit of capitalism), which had been ethically suspect in highly capitalist centers of the fourteenth century, could have been regarded as the essence of moral conduct in backwoods Pennsylvania in the eighteenth century. His conclusion that the development of Protestant religious thought had much to do with the change in values will survive the challenge of Samuelsson's provocative essay. Weber's subtle and erudite discussion of his limited theme still excels most of his critics even on points they think they have discovered.

J. William Fredrickson

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Economic History; Economic Development; National Economies

Possibilities of Economic Progress. By A. J. Youngson. New York: Cambridge University Press, 1959. Pp. x, 324. $6.00.

Professor Youngson has done a good deal more than contribute just another tome to the current outpouring on economic growth. His study is a landmark pointing the way by which the accretion of knowledge on economic growth must occur. He has thoughtfully combined the theoretical literature on economic growth with the insights derived from four case studies in economic history. The vast literature on economic growth has provided something less than a satisfactory body of hypotheses on the subject. It is likewise evident that economic history has contributed little to the field. The difficulty is that economists concerned with growth have paid far too little attention to the historical experience of economies, and that economic history has provided all too little of use to the student of economic growth. Yet clearly economic history should be the basic research field for economic growth if progress is to be achieved. Youngson's study gives an indication of the promise of such an approach. It also inevitably reflects the current shortcomings in these two fields.

Part I includes an able discussion of the difficulties involved in the meaning and measurement of economic growth whether conceived in welfare or productivity terms. In successive chapters he develops a series of hypotheses about economic growth with attention focused on three aspects: (1) invention and the adoption of new ideas (particularly as they lead to further innovation and investment); (2) the supply of enterprise; and (3) the "drift towards massive production" (primarily as a consequence of improved transport and economies of scale).

The four case studies in Part II on the acceleration of economic progress are: Great Britain, 1750-1800; Sweden, 1850-1880; Denmark, 1865-1900; and the Southern United States, 1929-1954. In Part III the author summarizes some of the conclusions that emerge from the application of the hypotheses developed in Part I to the case studies in economic history. While recognizing the diversity and certain unique aspects in each case, the author suggests that three important conclusions emerge. The first is the decisive role that involvement in the international economy played in each instance (in the rest of the United States in the case of the regional study of the American South). The second is the importance of agricultural reform, and the third is the complex interdependent pattern that development takes. Youngson also has some general comments drawn from the four studies on sources of capital and population and labor force changes.

In the final chapter some suggestions for the present are offered, drawn from the previous material. The author concludes that countries seeking accelerated development should: (1) think carefully about extending their international contacts (despite the risks involved) rather than conceive of development in national terms; (2) conceive of the role of government less in terms of undertaking specific development projects and more in terms of improving "the general conditions and equipment of economic life"; and (3) devote particular attention to agricultural development.

As noted above the shortcomings of the study are inherent in the state of the fields of economic growth and economic history. Until a better understanding of the interrelationships involved in economic growth is developed it is hard to be convinced that the strategic elements upon which attention is focused are indeed the critical ones, and the literature on economic growth is strikingly deficient in exploring these interrelationships in the past development of economies. The historical evidence suffers from the fact that it is too skimpy to bear the burden of proof and reflects the fact that economic historians have been derelict in not developing either the statistics or the generalizations about the development of economies which are needed for such case studies. Progress along the lines Youngson envisions initially entails fundamental research in both directions. His study points the way.

DOUGLASS C. NORTH

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Economic Planning in Underdeveloped Areas: Government and Business. By EDWARD S. MASON. New York: Fordham University Press, 1958. Pp. viii, 87. $2.50.

In this second series of Moorehouse I. X. Millar Lectures, Professor Mason has not lighted a very clear path for those interested in an answer to the question: How should the United States proceed in its efforts to raise per capita incomes around the world? Perhaps this is as it should be, but one cannot help but wonder if more light, as well as heat, would not be provided by the development of a definite stand. I am convinced that this approach would be much more helpful to the political leadership of the underdeveloped nations. I doubt that one can be all things to all people, and I am afraid that Mason attempts to be in these lectures.

The first and second lectures point out in general terms the underlying conditions in countries currently seeking rapid development from a low-income level. These prevailing conditions are then related to the pre-industrial revolution conditions in England, Japan, and the United States. The differences highlighted in this comparison are pessimistic, as they should be. The points included are the helpfulness of the legal system, the per capita income, the level of education, and government efficiency. The evaluation does not consider other comparisons that are also pessimistic. Examples that come to mind are birth rates and death rates, population base, consumption propensity, and political stability.

The third and fourth lectures consider government initiative in general and in Southeast Asia. The points developed in these two lectures give the impression of a strange indecision or ambivalence. On page 51, government is given a dominant role because of "technological and psychological requirements . . . and probable sources of funds. . . ." When actual government performance is discussed, such statements are made as "inevitable limitations of governmental capacity to manage and control" (p. 69); referring to India and Pakistan, "Whatever has been or can be said of the private sector, its record to date, if the projections of the various plans are to be taken seriously, has been substantially better than that of the public sector" (p. 75); and, again, referring to these two countries, ". . . despite recent increases in taxation, tax contributions to development will be small or negative" (p. 76).

Reconciliation, of a sort, of the dichotomy is made in the next to the last paragraph, where it is concluded that "large governmental participation in Asian development programs [may be] a temporary phenomenon associated with early stages of economic growth. . . ." Why this might be the situation is not explained beyond noting that it happened in Japan.

The title is much too broad, for concrete examples of the current development problems are drawn almost entirely from South and Southeast Asia. This was perhaps unavoidable because of the limits set for the lecture series, but a greater effort to select concrete examples giving an insight into cultural, environmental, and economic differences would have contributed much to justify the title and to increase true reader understanding.

However, these lectures do provide, in capsule and understandable form, a view of many of the relationships and current ideas (including basic reference sources) associated with the acceleration of economic development in low-income areas. This is the most useful aspect of the volume and certainly justified its publication.

R. W. Lindholm

University of Oregon

Saggio sullo sviluppo economico dei paesi arretrati. (An Essay on the Economic Development of Backward Countries.) By Vittorio Marrama. Turin: G. Einaudi, 1958. Pp. xii, 396. L. 3500.

This book serves, *inter alia,* as a critique of the best writings dealing with the economic development of underdeveloped countries. Its value as a reference as well is incidental. Though not a profound treatise, Marrama's work nonetheless represents an authoritative contribution to the subjects of economic development theory and policy.

Part I analyzes basic concepts such as underdevelopment, backwardness, and poverty; real per capita income, productivity per man-hour, and other indicators of the well-being of a people and the development level of their country. The problem then brought into focus is: Underdeveloped countries are characterized by average low per capita income and a high concentration of total income in the hands of a few.

Part II deals with factors contributing to economic backwardness. After noting the important extra-economic forces influencing development, such as customs and habits of peoples, their history, religious beliefs, and political and social systems, the author goes to the heart of his study. This concerns three decisive economic factors which are characteristic of underdeveloped countries: limited capacity to accumulate capital; limited capacity to import; and improper utilization of available investment resources and import capacity.

Underdeveloped countries lack the adequate infrastructural capital assets (roads, railways, electricity-generating plants, etc.) essential for their development. Moreover, the few manufacturing enterprises that may exist in these countries are isolated (i.e., outside of a general industrial environment) and do not benefit from external economies. Thus, the ratio of capital to output is greater in the underdeveloped than in the advanced country. Consequently, a greater investment increment is needed to insure a given increase in output. The low level of productivity, income, and savings renders capital accumulation difficult, and external assistance is therefore necessary to help to break the vicious circle.

Capital accumulation required for development in such countries depends primarily upon imports of equipment, etc., acquired with export earnings. Most underdeveloped countries are exporters of primary products, violent short-run fluctuations in the prices of which influence the countries' capacity to import. Prices of primary products have also suffered a secular decline. These backward countries have experienced a diminution of their capacity to import from decade to decade.

The final decisive factor that has contributed to the backwardness of these countries has been the utilization of investment resources and import capacity for unproductive purposes. The high rate of population growth necessitates diversion of a disproportionate share of resources to investment in dwellings. Since the marginal propensity to consume is high, the total of net savings is low. Both public and private investment of meager resources tends to be imitative of rich countries and goes into such unproductive projects as ornate public buildings, country clubs, luxury resorts, etc. The imitative principle applies also in the consumer-goods field, "interdependence of consumer preferences." The inhabitants of poor countries inordinately import luxury goods, travel abroad on expensive tours, and maintain savings in idle balances.

Fundamental economic development policy goals and measures for their attainment are discussed in Part III. The primary goal should be that of utilizing available resources so as to accelerate income formation. Among alternative avenues of investment those that promise increased production over the short term, or those with a low ratio of capital to income should be selected. Stimulation of savings is essential to capital formation, but since savings are insufficient in underdeveloped countries, capital must also be obtained from abroad through the World Bank, the Export Import Bank, etc.

The problem of increasing import capacity should be attacked by stabilizing world market prices for primary products through international control of supplies and, on a national level, by improving quality and types (diversification) of primary products for export. Other effective policies are utilization of seasonal and hidden unemployed in productive public projects, industrialization to provide substitutes for imports, and agrarian reform. Finally, policy should aim at redistribution of income without hampering the propensity to save. In backward countries this is necessarily identified with the reform of regressive tax systems.

Part IV considers various programs for rational economic development; the difficulties arising in their choice and implementation; an evaluation of their balance-of-payments and income-redistribution effects; the advantages and disadvantages of imput-output analysis for devising practical programs; criteria for comparing the income-producing capacity of various types of investment; and the problem of establishing priorities for implementing individual projects. Finally, an Appendix is devoted to a consideration of the economic development problems of Southern Italy as a case study. The author employs theoretical tools and statistical data with the skill of a mature and experienced researcher. The clarity and realism of exposition make the reader aware of the many factors which qualify the findings of abstract analysis. For example, he attacks the doctrinaire orthodoxy of neo-Malthusians who place too much emphasis on overpopulation as a negative factor contributing to

underdevelopment. Their formula for developing backward countries, namely "accumulation of capital" coupled with "birth control" is much too simple. They fail to identify the true obstacles to development. Excess population is but a symptom of the failure of backward countries to use economic resources properly. Highly industrialized countries have absorbed population increases without lowering their standards of living. Moreover in the 19th century these countries experienced a greater rate of population increase than did the backward or poor countries.

On the whole, this volume is a worthwhile addition to the evolving literature on the economics of development. It places at the reader's disposal the benefit of the author's research over the period of a decade on location in many backward countries of the world. The mass of difficult technical material is well organized and cast in an unusually facile style. It should be read by every economist and sociologist concerned with economic development problems.

NICHOLAS M. PETRUZZELLI

Washington, D.C.

Sviluppo economico e produttività del capitale. By AUGUSTO GRAZIANI. Naples: Eugenio Jovene, 1957. Pp. 136. L. 1250.

Augusto Graziani has added another thoughtful well-reasoned study to the growing list of volumes on economic development and the productivity of capital. Unfortunately there is nothing very new about what he has to say.

The author has attempted to find out how the productivity of capital varies in the long run in conjunction with the general theory of economic development. He considers short-run variations and business-cycle fluctuations only briefly. The treatment is orderly and well planned. After a couple of introductory chapters, Graziani develops a model of economic development, discusses two types of investment, the place of natural resources in development, the impact of technological changes, the effects of changes in distribution and the theories of Marx, Ricardo, Keynes and Hansen in this area. He concludes that "in the period of modern economics, the productivity of capital shows no tendency to decline" (p. 122). This is hardly a startling discovery. Also he says (as did Marshall) that the productivity of capital and its progress in development must be studied dynamically, not statically. Then, like Marshall, he proceeds to treat the problem in the usual static manner.

Graziani makes some helpful comments about the exhaustion of raw materials (always excepting land, which he considers inexhaustible). He says modern technology steps in with some newly discovered material, which replaces or improves upon the older raw material. Also in commenting on the stagnation theory, he says, "Although the theory of economic maturity is logically correct [?], it is based on two hypotheses that cannot be found in contemporary economic reality[!]" (p. 119).

The statistical evidence is put into an appendix. Graziani knows the literature very well and his statistics are mostly American. The reviewer has the feeling that the study is a skillful exercise in analysis, but that it is somewhat disappointing in its results.

HENRY S. MILLER

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British Economic Policy Since the War. By ANDREW SHONFIELD. Baltimore and London: Penguin Books, 1958. Pp. 288. 85¢.

Ever since the publication of Harrod's *Are These Hardships Necessary?*, the question of the adequacy of the level of capital formation has been in the forefront of all serious discussion of Britain's postwar economic problems. Andrew Shonfield, who is economic editor of the *Observer*, has made it the central issue of his highly polemical essay *British Economic Policy since the War*. Shonfield's basic theme is that Britain's postwar difficulties stem largely from the fact that her productive capacity has not grown sufficiently fast in the postwar period. This, in turn, is attributable to three factors: a misguided and disproportionately large defense program; postwar sterling area arrangements which permitted a (virtually) unimpeded outflow of capital from Britain to outer sterling-area countries; and the tendency of the postwar governments to cut back on the investment program whenever restraint in domestic expenditure was required to stay an intolerable drain on the reserves. The first two factors allegedly deprived the investment sector of resources while the third exercised a depressing effect on the marginal efficiency of capital. A large part of Shonfield's essay is devoted, therefore, to a plea for a reappraisal of policies in these areas to create a more hospitable environment for investment.

Shonfield describes his essay as concerned with "politics looked at from an economic viewpoint" (p. 11). To the extent that he emphasizes the economic costs of political decisions his characterization is appropriate. This reviewer has qualms, however, concerning the manner in which he arrives at his conclusions. By and large, Shonfield thinks it is sufficient to indicate that the economic costs of certain policies are high to condemn them. This procedure, however, is inappropriate. A valid analysis requires the development of criteria which would permit us to measure the marginal economic costs of political decisions against their marginal noneconomic advantages. In the absence of criteria of this nature, it is not permissible to conclude that because the military program or the sterling area arrangements tended to depress investment, these policies were inappropriate. As important as a high level of capital formation is, it cannot be assumed a priori that it must take precedence over everything else. While Shonfield may be correct, his conclusions remain mere assertions and do not logically follow from his premises.

The assertive nature of Shonfield's essay is also apparent in his blanket condemnation of British postwar governments for their general policy of restraining investment during crisis periods. He displays no awareness of Harrod's and Nurkse's analyses of this problem. Apart from the fact that capital formation was more readily controlled by the government than other components of domestic expenditure, the desire to maintain reasonably full employment was an important reason for operating on the investment sector. Given the nature of the resources required to produce capital goods for domestic use, domestic consumption goods and exports for which an extensive demand existed, a much larger reduction in consumption expenditures would have been required to effect a given improvement in the balance of payments than in investment demand. This is not to imply, of course, that the decision to restrain investment is beyond criticism. Perhaps too much emphasis was

placed on the maintenance of full employment in the postwar period. The only point I wish to make is that the problem is much more complex than Shonfield suggests in his essay.

Despite these criticisms, Shonfield's essay deserves a wide audience. It is a well-written, provocative and intelligent commentary on important problems facing Britain. And this is considerably more than can be said for much of the current literature directed to the elusive "intelligent layman."

ELLIOT ZUPNICK

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Canada's Economic Development, 1867-1953. By O. J. FIRESTONE. Income and Wealth Series VII. London: Bowes and Bowes, 1958. Pp. xxii, 384. 45s.

This volume is the result of a venturesome undertaking by Dr. Firestone assisted by two of his colleagues in the Department of Trade and Commerce in Ottawa, T. R. Vout and A. W. A. Lane. It is an attempt to interpret Canada's economic development over the 86 years since the confederation of the British North American colonies in terms of national product, national wealth and growth in the population and labor force. Since most of the relevant official estimates begin with the year 1926, Firestone and his colleagues in Ottawa prepared new and admittedly rough estimates of the following aggregates: (1) Births, deaths, marriages, emigration and net family formation on an annual basis from 1867 to the 'twenties; (2) Gross national product by industry in current and constant dollars for one year in each decade from 1870 to 1920; (3) Gross national expenditure and its major components for each year preceding the decennial census from 1870 to 1920 with the exception of 1880, again in current and constant dollars; (4) Various new price series required to deflate the national accounting aggregates; (5) Various supplementary series including the value of reproducible capital in key industries.

Conceptually, all of these aggregates are obviously useful in the interpretation of the country's economic development, but I do think the author errs in attempting such an interpretation on the basis of these pioneering quantitative results. I would have preferred a bare presentation of the estimates and a more extended discussion of the sources and procedures. The estimates are Firestone's significant and valuable contribution and their revision will indicate, as Simon Kuznets states in the preface to the volume, ". . . that Dr. Firestone and the others have provided stepping stones for further work in the field—an indispensable objective for an activity in which only cumulative and cooperative effort can yield significant results" (p. xv).

Users of these estimates will want particularly to read Sections 11 and 12 (Part III, pp. 235-326) in which the limitations of the data, the sources and the chief methods used are described and the basic tables presented. Part IV, which concludes the volume, is an historical review of earlier income and wealth estimates for Canada which should prove very useful to other investigators of Canada's growth.

Rather than attempt to discuss differences in the interpretation of Canada's economic development, it is more useful here to indicate two or three of the

major errors or inconsistencies—or, at least, what appear to me to be errors or inconsistencies—in the estimates.

According to the estimates, national income per capita in Canada increased at an average annual rate of 1.59 per cent from 1870 to 1890, 1.95 per cent from 1890 to 1910, but at a *zero* rate from 1910 to 1930 (p. 171). I find this hard to believe. There are official records of wages rates and of the cost of living from 1913 on. These data show an average annual rate of growth in real wages of 1.70 per cent from 1913 to 1930. This comparison suggests that Firestone's benchmark estimate for 1910 may be too high. His next benchmark is 1920 and his series ends in 1925. It is *assumed* that the series is consistent with the official estimates which begin in 1926 and the two discrete sets of estimates appear in numerous tables without comment on the problem of levels up to 1925 and since 1926. I am, therefore, particularly inclined to distrust comparisons of years before 1926 with years after.

Firestone's benchmark estimates of gross national product for 1870, 1880, etc. are based upon decennial censuses. Annual interpolations cannot add *new* information as to the *levels* of the series. Levels are determined as accurately as possible by the benchmarks. Annual interpolations that are then averaged can change the apparent level at different points in time significantly and undesirably. Firestone's method of interpolation is wrong in principle. Interpolators were constructed for each major component—consumption, domestic investment, etc.—and then combined with fixed weights. Among the results which I would question is a level of GNP in constant dollars at a considerably higher level during the latter 1870's than during the earlier '70's (p. 276), although Firestone himself refers to "a severe economic slump from 1874 to 1880 . . ." (p. 146).

There are also some major anomalies in the population estimates. The official record of birth registrations dates from 1920. Firestone's annual estimates for the years 1870 to 1920 are too low. The benchmarks are again tied to the decennial census. The estimated absolute level of births in each year preceding the census is equal to the reported number of children age 0 to 1 plus half of the reported deaths to children age 0 to 1 in the 12 months preceding the census. Both totals are undernumerated in the census by at least 15 per cent. My guess is based on the following:

Firestone shows a crude birth rate for Canada of 33.7 in 1870. As a matter of firm record, the crude rate for the Roman Catholic population of Quebec in that year was 45 per thousand. Weighting them as 1 of 3 in total population implies a crude rate of 28 per thousand for the rest of the population. The Canadian rate has been that high in recent years. In an unpublished paper "Long Swings in the Growth of Population and in Related Economic Variables," Simon Kuznets shows a crude rate for the United States in 1870 of 43.5 per thousand.

As a further check one can estimate the crude rates for the province of Quebec on Firestone's method for 1870, 1880 and 1890 and then subtract from this estimate the recorded births of the Roman Catholic population of Quebec. This yields total births for the remaining population of the province, who were 14 to 16 per cent of the total, of 246 in 1870, minus 2900 in 1880,

and minus 1425 in 1890. Since Firestone applied his method to all but the Roman Catholic population of Quebec, the underestimate originating in the underenumeration of the census applies to all but the Roman Catholic population of Quebec. By a similar method one can check Firestone's implied crude death rates for non-Catholic Quebec and the rest of the country. The rate for 1870 is 15.6 which appears to me to be too low. The Canadian rate in 1920 was 13.3. Kuznets' estimate for the United States in 1870 is 25.7.

One would expect the underenumeration of total deaths in the census to be relatively less than of deaths to infants and of children age 0 to 1. This expectation is borne out by Firestone's estimates of net migration which were obtained as a residual. For example, his estimate of the net out-migration from 1870 to 1900 is 175,000. This estimate is much lower than the estimate of 472,000 obtained by Nathan Keyfitz ("The Growth of Canadian Population," *Population Stud.*, 1950-51, *4*, 47-63). Keyfitz used English life tables to project Canadian life tables to the earlier census years.

These few comments will serve to illustrate the hazards of taking individual series too seriously. Some of the series do stand up very well in the uses that can be made of them. In addition to the stimulation of further work in this field and subsequent revision and improvement of the estimates, Firestone's estimates, like all contributions in this field, have another valid use. If a wide variety of these historical data appears to support particular hypotheses, one's willingness to entertain them seriously is strengthened even though the individual series are known to be in various ways imperfect. In the words of the Preface, "One can only express deep appreciation of the contributions thus made to the stock of our knowledge on the quantitative aspects of economic growth. . . ."

KENNETH BUCKLEY

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The New Economy of China. By GYAN CHAND. Bombay: Vora & Co., 1958. Pp. xiv, 429. Rs 16.00.

Described as a "factual account, analysis and interpretation" of the economic development of mainland China, this book represents the observations of an Indian economist who was head of the Economic Section of the secretariat of the Indian cabinet in 1948-51. The author attempts to discuss in sixteen chapters such topics as land reform and the rural economy, the marketing and producers' cooperatives, industrial expansion, labor, commerce and communications, money, banking and finance, Soviet aid, and the population problem. The entire account is meant to produce a "thorough understanding of the processes at work" in the Chinese economy.

In spite of its broad scope, the book can hardly be recommended either as a source of reference or as an unbiased and comprehensive study to introduce the nonspecialist to the Chinese economy. In the first place, there are certain noteworthy omissions both of subject matter and of facts, examples of which are, *inter alia*, the serious impact of losses to Manchurian industry, through the postwar stripping by Soviet troops, on China's economic rehabilitation and monetary stabilization (Ch. 14) and the reappearance of price instability

following the outbreak of the Korean war (Ch. 12). The Manchurian losses, estimated at $2 billion by the Pauley Commission, would, if mentioned, cast a different light on the author's estimate of 4.22 billion rubles of Soviet aid to China (pp. 359-60). More serious, however, is the lack of any informed discussion of the size of the Chinese national product and its allocation between investment and consumption. Although the book was published in April 1958, most of its rather scanty statistical information refers to the period before 1954. Moreover, granted that it is most difficult to separate the political from the economic in the case of this subject, it is nevertheless somewhat surprising to find a scholar naïvely describing the Chinese scene as one in which "democracy as a spirit of true fellowship and cooperation is a reality" (p. 399), or that "the historical fact is that without the Marxist party the benefits, which the present regime has undeniably conferred upon the people, would not have in fact accrued to them" (p. 388). An experienced economist though he is, he has apparently taken at face value much of the official interpretation he received during his six-month stay in China.

The redeeming merit of the book lies solely in its being an interpretation of Chinese economic development through Indian eyes. Even though the author purposely refrains from drawing explicit comparisons between the two countries, for the Western reader the following points are well worth consideration. First, Gyan Chand believes that in China the development of handicraft cottage industry may continue to expand along with that of factory production and that the former will not suffer from the competition of the latter because they serve different consumer markets and because the output of handicraft production is channeled through the trading cooperatives. "The whole program is now being put into operation on the assumption that most of the consumer goods, particularly in the rural areas, are to be produced by the crafts . . ." (p. 188). More recent developments, such as the mushrooming growth of the "back-yard blast furnaces" and the expansion of native industry in the rural "communes," show that the utilization of these industries has been increasingly extended to the production of producer goods. Needless to say, such developments would help to alleviate the problem of rural underemployment. Secondly, in the author's opinion, notwithstanding the Communist rejection of the Malthusian population theory as a direct negation of the Marxist faith, it is necessary to engage in a critical re-examination of population policy. This re-examination is recommended not merely to Communist China, but also to Australia, Canada, the United States and certain Latin American countries (p. 373). The implicit suggestion as a solution of China's population problem is, of course, emigration. If this is a provocative thought, one might note further the author's belief that through trading with the Soviet Union under long-term agreements China has been freed from the effect of fluctuations in world prices and has concurrently been assured of expanding markets for her exports. The Soviet Union, it is said, has been prepared to gear her production "especially to meet the specific requirements of China" (p. 356). Finally, the author asserts that the success of China's economic development to date is largely due to an increase in the vertical mobility of talents (p. 398) and the

large-scale participation by the population in the various facets of economic development (p. 395). Both conclusions are probably true. But the disturbing thought is his contention that all this would not have happened but for the Communist revolution.

YUAN-LI WU

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Colonial Planning—A Comparative Study. By BARBU NICULESCU. London: George Allen & Unwin, 1958. Pp. 208. 18s.

This reworking of a doctoral dissertation is a comparative study of economic planning in areas that are or recently were colonies. It deals primarily with British territories, rather briefly with French, and quite skimpily with Netherlands and Belgian colonies. Its coverage is very broad. In about 165 pages of actual text, some seventy-odd territories are "touched upon" and the "development plans of some sixty of these territories have been taken into account" (pp. 13-14).

The book's scope is wide also in that it deals with many aspects of the subject. After a one-chapter introduction, Part II, "Background to Planning," discusses in a largely conventional way a number of problems and possible policies related to colonial economic planning. The topics examined seem to be chosen rather randomly from among a large number that would be of equal interest. Part III consists of a useful one-chapter historical survey treating "The Growth of the Idea of Development Planning for Colonial Territories." In Part IV, "Planning Machineries," the planning process is divided into four stages: (1) collection of the required data; (2) "setting up of planning organizations" or "deciding on priorities" (defined both ways presumably because the planning organizations decide on priorities); (3) implementation of the plan; (4) preparation for the new plan—a transitional stage linking one plan with the next. Part V, grandly entitled "Analysis of the Plans," is concerned chiefly with the problem of priorities in development. The discussion of priorities is centered upon the relative merits of social welfare and economic development expenditures, even though the author subsequently (p. 182) finds it "difficult to accept the existing dichotomy into *economic* and *welfare* planning as economically valid. . . ." The rest of the section touches upon the length of the planning periods, the sources of funds, and rather commonplace discussions of communications and agriculture. The concluding Part discusses briefly some noncontroversial achievements of the plans and some criticisms which, since they are dealt with on such a general level, the author finds difficult to appraise.

In his concluding chapter the author seems to recognize the two major shortcomings of his book. First, the analysis is rather pedestrian; one looks in vain for stimulating interpretations or penetrating insights. "The analysis attempted in the course of this book cannot be said to have revealed any striking new facts or interpretations in connection with development planning in the colonial territories surveyed" (p. 177). Second, the enormous range of the book seems to require generalizations so broad that they have little signifi-

cance. "Plans, their contents, and their effects have varied from colony to colony. General appreciations have therefore of necessity to deal with rather vague overall concepts" (p. 179).

On the other hand, the book provides a serviceable general picture of colonial economic planning. Its discussion of the administrative problems encountered and their effect on the nature of the plans is excellent (Ch. 3 and *passim*). The book is useful for those interested in the economic development of one (or more) of the present or recent colonial areas.

SAYRE P. SCHATZ

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Population Growth and Economic Development in Low-Income Countries. By A. J. COALE and E. M. HOOVER. Princeton: Princeton University Press, 1958. Pp. xxi, 389. $8.50.

This is another of the major studies of population change in the countries of Asia carried on by the Office of Population Research of Princeton University. Unlike the previous works, which have been historical and empirical, this one is primarily valuable for its theoretical model. India is essentially used as a laboratory test of the theory—which is at the end also briefly tested on Mexico as a country with a quite different starting population base, and with different policies, in order to show its relevance under other conditions. The authors carefully and rightly disavow that the book is either "an appraisal of Indian plans, or a prediction of the future rate of economic growth." (The subtitle of the book—"A Case Study of India's Prospects"—is therefore misleading.) In the course of the test the authors develop various projections of India's population to 1986 and beyond, and present an excellent summary and analysis of India's second five-year plan and prospects for the future growth—but these are really by-products of the theory.

The theory may be summarized briefly although necessarily crudely. (The authors themselves provide such summaries frequently.) Let us assume that the low-income countries start with high birth rates and high death rates which keep the population in a low-income equilibrium. Under the impact of the recent health-improving developments, such as DDT, it is possible to reduce the mortality rates, not slowly (as former theories implied) but very rapidly, and before or simultaneously with industrialization and urbanization. Previously, it was assumed that the decline in fertility lagged behind that of mortality, creating a population explosion; but that gradually a new equilibrium was established at which the greater population was maintained at a higher level of per capita incomes. Now, in the low-income countries the speed of the decline in mortality rates and the delayed reaction in fertility rates is such that the rate of population growth becomes very rapid—on the order of 2 to 3 per cent per annum. During the period of rapid population growth, assuming no migration, there is a diversion of resources from directly productive investment to investment in late-yielding social expenditures to support the growing population—which in the early period is largely below working age. If fertility rates could be reduced, more might be devoted to either direct productive investment, or to higher per capita welfare (health and education)

expenditures. Thus lower rates of fertility have as direct, short-run consequences higher investment, higher per capita incomes, and improved well-being of the population; and as a longer-run cumulative result, continued higher rates of growth of per capita incomes than in the case of higher fertility. This may be crucial for the long-run ability of the country to support the rapidly growing population under either fertility assumption. (In these low-income countries the stimulating effects of population growth from more births as a demand creator in the Keynesian sense, is either nonexistent if the population is already very large, or is outweighed by its effects upon investment.)

As a test of this model, the authors develop a series of population projections for India on the basis of past trends (reinterpreted) and certain assumptions with respect to future mortality and fertility rates. The projected mortality rates are based conservatively upon the experience of other countries, especially Ceylon, which have introduced health programs similar to those already introduced or planned for India. The fertility estimates are on three different assumptions: maintenance of present rates, an immediate decline in fertility, and a delayed decline. These are then used to project total population growth and age composition. (These population estimates, which have been available in India in an earlier draft for several years, have already had the effect of raising previous projections in India for planning; the discussion of the Indian third five-year plan assumes a rise in population of 2 per cent per annum.)

It is assumed of course that India will be able to sustain the projected rates of population growth at some level for a period of time—at least the thirty years to 1986. The authors, on this assumption, and assuming technology about constant and a closed economy, analyze the effects of the varying rates of population growth upon India's economic development. The second five-year plan is used as the basis of this analysis. (Unfortunately events move so rapidly in India that the second plan is no longer a satisfactory basis for projection—however this is not too important as a test for the model, although it would be very important if the primary purpose of the book were a projection of the Indian economy. For example, one area of discussion in this book in which the second plan is a misleading guide is the analysis of the community development programs and agriculture. Use of the second five-year plan as a basis for this analysis makes projections of trends in farm output somewhat too optimistic; this of course reinforces the implications of the theoretical model.)

The economic analysis contains very interesting discussions of certain general problems of underdeveloped areas such as capital-output ratios, investment and saving, and shows an imaginative use of the Cobb-Douglas function for income projection purposes—all useful in general analysis of other economies. This analysis is used to project investment and income in India over the thirty-year period. It is conclusively shown that with lower fertilities a greater proportion of national income would be spent as investment for growth and this results both in higher per capita consumer income, and a higher rate of growth in total output and per capita income during the period. The authors have deliberately assumed, in their economic projections, a closed economy for India. In the long run, but possibly even before 1986 if the projec-

tions of agricultural output prove to be too optimistic, and certainly beyond it, this may prove quite unrealistic. The discussion of Mexico shows that similar conclusions hold in a smaller developing economy with a greater volume of foreign trade than India, and with very different starting conditions.

The authors are to be congratulated on the very neat construction of a theoretical framework and the testing of that framework in a real situation. For the lay reader it may be too logical and dry in its construction and style. However, as an example of methodology, it is almost a model of economic-demographic analysis; in substantive terms it will be necessary reading for anyone interested in the relationship between population growth and economic change.

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Le libéralisme économique et les pays sous-développés: études sur l'évolution d'une idée. By Frédéric Clairmonte. Geneva: E. Droz; Paris: Minard, 1958. Pp. 361. 30 sw. fr.

Dr. Clairmonte has written a scholarly, provocative volume on the relation of classical *laissez-faire* economic theory to the critical need for industrialization of the underdeveloped countries of Asia, Africa, and Latin America. He characterizes as underdeveloped those countries that lack modern technology and the socio-economic superstructure that is built upon that technical base. In so doing, he has many precedents, but theoretically he begs the question as to what general factors or criteria determine underdevelopment. He believes no adequate, general theory of economic growth has as yet been presented.

With considerable expository skill and an intense dislike of British industrial capitalism, Clairmonte portrays "classical economics" from Ricardo to Marshall and Pigou as an honest rationalization of British middle-class economic needs during the nineteenth and early twentieth centuries. He eulogizes the strong defense of nationalism, protectionism, and industrialization by Alexander Hamilton, Matthew Carey, and Friedrich List. List is given the highest praise for his attacks upon British economic doctrines, his role in bringing the *Zollverien* into existence, and his interpretation of history as a dialectical process.

As part of his case against the classical economists and their followers, Clairmonte gives a devastating account of the harm done to India during the eighteenth and nineteenth centuries by British innovations in manufacturing, agriculture, and land-ownership. A good deal of this indictment seems sound. Yet I think the evidence and analyses of other authorities justify the conclusion that British rule in India brought a differential diffusion of modernization: progress in certain key sectors of Indian economy and society, stagnation or retrogression in certain others, with a net long-term increase in real national income that was accompanied by so large a population increase as to keep the per capita real income down to either a stationary level or a slow rise over many years. Moreover, considerable responsibility for the retarded

economic progress in India rests with the native population of India for having preserved institutional obstacles to economic progress and social mobility.[1]

Clairmonte demonstrates the intimate relationship among foreign trade, foreign investment, and the progress in industrialization of the major Western European and American powers in the third section of his book. He admits that foreign investments resulted in the increased welfare of Europe, the United States, Canada, and Australia. Yet he maintains that western capitalists did not tackle the crucial problems or elicit the latent human talents of the native populations of Africa and Asia. This time he singles out nineteenth-century China as an example of how Western factory goods could come in too speedily and injure the handicraft industries of the non-European poor countries. Here a mass of evidence supports his main charge. But Western enterprise also did much to modernize China's industry, transportation, and education and eventually to raise the Chinese standard of living over the previously incredible low level.[2]

In the last quarter of his study Clairmonte rightly attributes the death of *laissez-faire* as the dominant economic theory and the world-wide spread of state-interventionism to the economic, political, and social changes following the first world war, the nationalistic rivalries of the 1920's, and the great depression of the 'thirties. He regards the emergence of the Chamberlin-Robinson theories of monopolistic, imperfect competition and Keynes's *General Theory* as the Western academic recognition of the inadequacies of classical economics in the realms of theory and practice. But he feels that the case for state-interventionism in the underdeveloped countries of Africa, Asia, and Latin America is far stronger than in those highly developed countries where full employment is the main concern of the governments. The crying need of the poor countries, in his eyes, is for industrialization, population control, national integration, and planning on a physical basis. He believes an enlargement of the public sector of these economies is an economic necessity and thinks that the governments of these former colonial areas will not welcome private foreign investments because these might result in high profits for foreign capital and would hinder the drive towards socialism.

The author is conscious of the difficulties in the realization of his project in the Middle East and Latin America from corrupt monarchies and anti-industrially oriented aristocracies; yet he is hopeful of progress. I fear, however, that there are more dangers arising from his emphasis upon speedy industrialization and upon the state as the chief instrument of economic change than he is conscious of. State enterprise and planning are necessary, but it is equally vital in these regions to build up an independent middle class in industry and agriculture if the perils of past despotic governments are not to

[1] Vera Anstey, *Economic Development of India*, 4th ed. (New York, London, Toronto, 1952), pp. 433ff.; Kingsley Davis, "Social and Demographic Aspects of Economic Development in India," and D. R. Gadgil, "Indian Economic Organization" in Simon Kuznets *et al.*, eds., *Economic Growth: Brazil, India, Japan* (Durham, N.C., 1955), pp. 263-315, 448-63.

[2] Cf. G. C. Allen and A. G. Donnithorne, *Western Enterprise in Far Eastern Economic Development* (New York, 1954), pp. 241ff and Marion Levy, "Contrasting Factors in the Modernization of China and Japan," in Kuznets, *op. cit.*, pp. 496-536.

be revived.[3] But there are limits also to foreign government loans from the rich to the poor nations, and private capital investment will have to be relied upon for many reasons, political and economic. I wish that the desirability of emulating the Danish producer cooperatives in backward areas had been stressed and that the problem of world-trade adjustment between the rich and poor countries had been analyzed at greater length.[4] Nevertheless, we are indebted to Clairmonte for a valuable exploration in the history of economic ideas and institutions.

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Poverty and Capital Development in India. By D. K. RANGNEKAR. London: Oxford University Press, for Royal Institute of International Affairs, 1958. Pp. xii, 316. $6.75.

A record and appraisal of public and private investment in postwar India, Mr. Rangnekar's study includes four chapters on agriculture, two on industry, two on transport and communications, and one each on housing, the level and pattern of aggregate investment, investment and progress under the first five-year plan, and the investment pattern and priorities under the second five-year plan. Chapters dealing with different sectors of the economy are chiefly statistical, with theoretical and other argument interspersed from time to time. In the much more readable final section, analysis of policy plays an increasingly important role, being central to the discussion of the second five-year plan launched in 1956.

Pessimists and critics of Indian economic policy will find much in the book to support their views. Thus, with respect to record and prospects: In spite of a substantial rise in industrial output per capita, average real income was probably still (1957) below the prewar level (pp. 100, 277). Total agricultural production appeared to be lower than before the war; the rise in reported output in recent years has been chiefly attributable to more favorable monsoons and better statistical coverage (pp. 40, 47). Moreover, the rise in the industrial production has not brought with it an increase in factory employment. Urban unemployment has been growing and population pressure on agricultural land has become greater than ever (pp. 80, 104-4, 256). Housing has lagged behind the growing population in both rural and urban areas; even the cities' middle classes are worse housed than some years ago (p. 180 ff.). There are heavy arrears in railway maintenance and replacement (p. 175). If India is to break through the vicious circle of poverty, savings must rise from the 6-7 per cent of national income achieved under the first five-year plan to at least 8-9 per cent, a formidable figure for a poverty-stricken country with a rapidly rising population (p. 216).

Rangnekar's criticisms of Indian policy include: Although the first five-year plan rightly assigned first priority to agriculture, it did not draw up a

[3] Cf. Karl A. Wittfogel, *Oriental Despotism* (New Haven, 1957), pp. 413ff.

[4] Cf. A. O. Hirschman, *The Strategy of Economic Development* (New Haven, 1958), pp. 185ff and C. P. Kindleberger, *Economic Development* (New York, 1958), pp. 238ff.

"sound order" of specific priorities, understressed transport and communications, and put too much emphasis on grandiose, spectacular projects involving excessive costs (pp. 26-27, 110-14, 175-77). The second five-year plan, expressing the ambitions of India's "articulate classes," is much less realistic than its predecessor in that it puts even more emphasis on the spectacular, concentrates on manufacturing (especially large-scale, heavy industry), assumes a rate of saving that almost certainly cannot be achieved, and relies on a degree of foreign aid that Indian policies make quite unlikely (pp. 249-76). Instead of pushing nationalization and reform measures intended to introduce a "socialistic pattern of society," the government should concentrate on providing the necessary "social overhead capital" (an enormous task in itself), encourage private enterprise to undertake industrial and other investment, and so modify taxes and restrictions as to give private enterprise both the means and the incentive to move ahead (pp. 117-22, 240-41, 273-75, 280-81). The private enterprise to be encouraged should include foreign as well as domestic business, since India badly needs foreign capital and expertise and receives very little under its present program (pp. 225-26, 255).

Since the book discusses so many problems, it obviously cannot thoroughly explore them all. Some of Ranknekar's rather sweeping condemnations of Indian policy, however, arouse counterquestions. For example: To what extent would funds which taxes now channel into public investment otherwise go into consumption or forms of investment not especially desired by the planners? What policies could India pursue which, in the present state of the world, would attract large sums of foreign private capital? As Rangnekar points out, this is in part a problem of Indian domestic politics, involving the ambitions and sentiments of India's "articulate classes." But the task of attracting foreign capital might not be simple even if Nehru were free to shape policy as he pleased and were not himself imbued with socialist and nationalist ideas.

The book's major shortcoming, an inadequate factual base, is not attributable to the author: The preface points out that much of the study's detailed information stops with 1951-52, and numerous other passages refer to the absence of data and the "guesstimate" nature of some published statistics. So long as Indian series remain thus faulty, intertemporal comparisons and judgments of economic progress or decay must be very broad or highly tentative. The folly of attempting precise comparisons between Indian and non-Indian levels of real income, investment and wealth is even more obvious, and Rangnekar is cautious when he compares India with other lands (pp. 2-4, 215).

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Japan's Postwar Economy. By Jerome B. Cohen. Bloomington: Indiana University Press, 1958. Pp. xiv, 262. $6.50.

Mr. Cohen's record of scholarship on Japan makes the appearance of a new work by him an event to be warmly welcomed. Readers of his past studies[1] will

[1] *Japan's Economy in War and Reconstruction,* Minneapolis 1949; *Economic Problems of Free Japan,* Princeton, N.J. 1952; as well as numerous articles.

not be disappointed in his latest work, *Japan's Postwar Economy* which covers the decade, 1946-1956, a decade in which Japan staged an amazing comeback.

In his preface Cohen describes his study as "a brief analysis of the nature of and factors responsible for Japan's economic recovery and of the economic problems which confront Japan." Actually, however, the focus is on the latter, the "economic problems which confront Japan." One chapter is devoted to analyzing the recovery, five to the major economic problems before the country. Inasmuch as trade is pivotal to the Japanese economy, Cohen spends three additional chapters delineating the character of traditional and probable future trade relations between Japan and the United States, Japan and Southeast Asia and Japan and the Communist Bloc.

The questions which Cohen sees as crucial for Japan are (p. 26): "How to produce more food and fewer people; How to employ 800,000 more persons each year; How can Japanese export prices be made competitive with those of West Germany and of other countries; How can Japan sell a billion dollars more of exports abroad; How can Japan achieve stable economic growth without inflation; How can substantial additional industrial expansion be achieved without bringing on recurrent balance of payment crises."

He builds his analysis on detailed factual data. (Unfortunately, footnotes are at the back of the book so that one is never-endingly turning pages.) Study of the data leads him to feel qualified optimism as to Japan's ability to overcome the formidable problems before her. His analysis is persuasive. In only one area does his presentation seem to this reviewer inadequate. Energy sources are obviously of critical importance to the argument. Cohen considers in turn hydroelectric power, petroleum and coal. Japanese coal production has been singularly slow to recover, and he takes as a datum that little can be looked for from added output of this industry. Undoubtedly he has a good basis for this position but inasmuch as the output of domestic energy figures prominently in his analysis, the reasons for the poor recovery and the probable poor future showing need to be stated.

In the reviewer's judgment, the argument of the book would have been clearer if the first chapter "Asia, Japan and the West," intended to provide setting, had been omitted. The chapter is essentially a collection of facts, and when one does not have the guideline of an argument, one can get quite hopelessly lost in factual data. Further with respect to organization, Chapter 11, entitled "Countervailing Forces" (contervailing to the reform forces of the Occupation), would appear to need another location in the book or it might have been given independent status, such as a journal article. In this chapter, Cohen paints in broad strokes the institutional framework of the economy the contours of which are beginning to emerge now that some time has passed since the ending of the Occupation. Inasmuch as this institutional setting obviously affects the "answers" which the economy is giving, this chapter could well have been placed at the beginning. "Countervailing Forces" does not purport to be a comprehensive summing up of Occupation efforts. It treats in summary form the demise of the military, the changes in the bureaucracy and the *zaibatsu* and the emergence of labor as a political force.

The Appendix contains a most useful reading list on Japanese economic development and current economic problems. Persons who followed events in considerable detail during the Occupation period but have not been in a position to keep up with the current literature will find the bibliography on current materials especially useful.

In 1958 there appeared not only Cohen's book but also G. C. Allen's *Japan's Economic Recovery* (London: Royal Institute of International Affairs, Oxford University Press). Although the two titles would suggest that the authors were treating the same subject (and in the same year), actually there is little duplication. Cohen's study is an analysis of future trends; Allen's work, a summing up of developments in the major sectors of the economy, is descriptive.

Analyzing the possible, probable course of an economy is no small assignment. All of the fields of economics are called into play. Cohen has performed the task with distinction.

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Zagadnienia socjalistycznej industrializacji Polski. (Problems of Socialist Industrialization of Poland.) By ANDRZEJ KARPINSKI. Warsaw: Panstwowe Wydawnictwa Gospodarcze (State Economic Publications), 1958. Pp. 239.

A young Polish economist provides the first assessment of the policies, implications and results of Poland's 6-year plan, 1950-1955. Karpinski accepts the policy of industrialization on the ground that it ensures a faster possible rate of development of the economy than the alternative concentration on agricultural production; for under Polish conditions the latter would require a considerable intensification of agricultural methods, which would itself be dependent on an industrial background. Existence of an agricultural overpopulation at the outset of the 6-year plan showed also the need to create alternative sources of employment where the marginal productivity of labor would be higher than in agriculture. Thus a policy of industrialization serves ultimately to raise the standard of living of the population, although, particularly during the first stages of industrialization, the relation cannot be one of direct casuality.

The 6-year plan achieved a substantial expansion of total production, and created a basis for further development. Industrial production rose during the period according to Karpinski by at least 120-130 per cent (as against 170 per cent in the official estimate based on gross production), an average growth of 15 per cent per year, which is higher than that of any West European country. The Polish rate of growth in the production of particular commodities, when compared at an equal stage of absolute production, was also higher than that in the Western countries. This achievement was, in Karpinski's view, the result of a high rate of investment possible under conditions of socialized ownership of means of production, and of a concentration on investments in industry to the extent of about 46 per cent of all investments.

The basis for further industrial development has been created by an expansion of key industries: metalurgical, machine-making and chemicals which between them accounted for 51.7 per cent of the total investments in industry.

Karpinski is not, however, preoccupied mainly with listing the achievements of the 6-year plan. In fact, much of his book is devoted to a critical analysis of its mistakes and of difficulties encountered. Instead of the heralded increase of real wages by 40 per cent, they showed virtually no rise between 1950 and 1955, while during the period 1951-1953 actually some decline took place. The plan did not bring about the results expected partly because of much higher costs of investment than those planned: the planned outlays covered only 62 per cent of the planned volume of investments. Industrial production rose at a rate slower than laid out for it in the economic plan. As causes of this inadequate performance, originating partly in too "mechanical a transplantation of Soviet experience" to Poland, Karpinski lists the following:

1. Too high a share of national income was devoted to investment, particularly at the low Polish level of income. The share of gross investment in national income in 1950 prices rose from 22.7 per cent in 1949 to 38 per cent in 1953. Because of the use of regulated low prices for investment goods, the actual share of accumulation in the national income might have been even higher.

2. Improper agricultural policy. While a rise in agricultural supplies is a precondition of industrial development, the policy of collectivization caused serious difficulties and either declines in production or declines in marketable surpluses. At the time when the plan envisaged a rise in agricultural production by 50 per cent and industrialization extended the market for agricultural commodities, the policy of forceful collectivization resulted in abandonment of farms, virtual elimination of private investment in agriculture, and in land being left uncultivated, all this when greater intensification of agriculture was called for. New investment in agriculture was thus left solely to the state, reducing the funds available for the industrialization drive, and causing a general all-round insufficiency of investment in view of the 6-year-plan goals. Karpinski draws from this an interesting conclusion that in an underdeveloped country the solution of the industrialization problem should precede the collectivization of agriculture.

3. Unwarranted dispersion of investment efforts, in part due to the need for state investment in agriculture, and in part shown in the pursuance of too many investment goals in industry. The plan was originally self-contained, embraced a whole investment cycle. Owing to higher than planned costs, to additional and unplanned investments in armaments industry, and to a slow gestation of new investments many projects on which the plan relied for its effects had to be postponed to the following planning period, or even to the 1960's, leaving the investment cycle uncompleted. A sample of completed investment projects mentioned by Karpinski shows an average delay of about two years. In practice completion of an investment project brought also smaller than its planned performance. Karpinski demonstrates that the gestation period of investments was longer in Poland than in the Western countries, and he places the blame for this outcome upon lack of sufficient concentration

of the investment effort and the harmful procedure of uniform reductions in investment funds allocated to a large number of projects, rather than greater concentration of investable funds. He also blames excessive bureaucratism which limits the initiative of investors, an acute scarcity of construction materials, and a low standard of organization of investment work.

4. Excessive concentration of investments in heavy industry. Within the price system implicitly accepted by Karpinski, characterized by low prices of producer goods, the economic effectiveness of investments, measured by the value of investment necessary to bring about a unit value increase in annual production, was lowest in heavy industry and highest in consumer goods industries. As the result of faulty planning and of the armament drive actually over 85 per cent of investment funds devoted to industrial development were directed to heavy industry, at the cost of development of consumer goods industries.

5. Excessive reliance on the construction of new establishments, instead of the enlargement of old ones which, on the average, showed greater economic effectiveness. Exceptions to this general observation constitute enterprises in which technological considerations make expansion of existing plants difficult (e.g., steel mills), or enterprises which use rapidly changing technology.

6. Errors in employment policy, consisting in (a) locating new establishments in labor-deficit areas, (b) precipitating a decline in handicraft and cottage industries, suited to small localities which had characteristically a surplus of labor, (c) causing too rapid an outflow of labor from agriculture to industry, with adverse social and economic effects, and (d) using extensive methods in raising the level of total production, rather than striving to increase the productivity of labor. Karpinski shows that in many new plants labor productivity was substantially below that in old plants.

7. Faulty premises employed in the development of foreign trade, primarily to the effect that the importation of raw materials would decrease with progressing industrialization, that the increase in exports could rely on agricultural products, instead of on a broad export program, and that equilibrating of the balance of payments could be achieved through an extensive anti-import program rather than through the development of exports.

The area of Karpinski's study is broad, his statistical coverage adequate, and his discussion illustrated by many revealing examples. His discussion of the errors of the 6-year plan leads him to the formulation of useful proposals for future economic policy, while he at the same time avoids doctrinaire thinking. Some of Karpinski's conclusions run parallel to changes in the economic policy embodied in the current 5-year plan. Karpinski uses, naturally enough, official Polish statistics which sometimes yield different indicators than Western concepts and statistical methods, and he displays too uncritical an attitude towards the official explanations of economic phenomena (e.g., unemployment statistics), but in the main his analysis and conclusions are the product of a thoughtful effort to explain the immediate past for the benefit of the immediate future.

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Finansy i sotsialisticheskoye stroitel'stvo, 1917-57. (Finance and Socialist Construction, Collected Articles.) Moscow: Gosfinizdat, 1957. Pp. 357.

The book under review, published to commemorate the 40th anniversary of the Soviet regime, includes 7 articles. The introductory and basic article, "Soviet Finances for 40 Years," presents a general picture of the development of the financial system of the USSR. This article is of particular interest because it presents and discusses figures pertaining to the military budget of the Soviet Union. It gives an idea of how grossly perverted are the Soviet data on military expenditures. It is well known that the published Soviet budget presents only the direct military expenditures, listed under the Ministry of Defense. These have decreased from 108.8 billion rubles in 1952 to 96.3 billion rubles in 1958, while their relative weight in the total budget decreased from 23.6 to 15.3 per cent. By comparison with countries of Western Europe and the United States, the proportion of the budget in the USSR destined for military use is extremely low.

Actually, however, both the military expenditures and the percentage they constitute of the total budget of the Soviet Union are considerably higher, being in large part included in other sections of the state budget. We do not know precisely how large are the concealed expenditures slated for military purposes. We do know, however, that they exist under such headings as education, various economic and socio-cultural undertakings, or simply under "other expenditures." For example, prior to the second world war (1937-40), of the total amount of capital investment allotted for industrial development, about 30 per cent went into war industries (aviation, ammunition, armament, etc.).

Increasingly large sums are being expended on atomic and other scientific research. Although these activities are directly associated with military potential of the Soviet Union, the necessary funds are included under the general heading of "scientific research." During the past 18 years the budget for this category increased from 1.1 to 15.0 billion rubles, or from 0.6 per cent (1940) to 2.4 per cent (1958) of the total state budget.

Another important factor is that in the Soviet Union there is but one budget that covers the needs of a Union Republic (the largest administrative unit) as well as the budget requirements of a rural Soviet. It includes many expenditures which have no place in a national budget of other countries, such as the financing of various economic and commercial undertakings. Once this factor is taken into account, the 1958 military expenditures of the USSR (excluding the budgets of the Union Republics) amounted not to 15.3 per cent but perhaps almost triple this amount.

During the past 8 years, any decrease in the stated military expenditures of the Ministry of Defense of the USSR may have been matched by an increase in concealed military expenditure items, or simply by a decrease in the purchase price of armaments and military equipment. Although we may only presume that this is the case, it is important to remember these peculiarities of the Soviet financial system.

The next 3 articles discuss the role of finance in the development of Soviet

industry, agriculture and the socio-cultural fields (education, science, medicine, social security). The fourth article stresses the increasing role of the budgets of the Union republics, since the adoption of the reforms which placed a large share of the administrative responsibility for industry and construction directly on the Union Republics. Of the two final articles, "Monetary System of the Soviet Government" and "International Currency and Financial Relations," the latter is undoubtedly of greater interest, since it includes the latest data on the allocation of loans and credits by the Soviet Union.

Theoretical questions, such as price formation and price policy in a planned economy, interrelationship between the budget and national income, and other aspects of Soviet finance are either omitted or discussed very superficially in these articles. Soviet publications concerned with social problems usually are of little interest and value. This book is no exception.

T. Sosnovy

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Economic Systems; Planning and Reform; Cooperation

System jugoslowianski z bliska. (The Yugoslav System at Close Range.) By Wlodzimierz Brus and Szymon Jakubowicz. Warsaw: Polskie Wydawnictwa Gospodarcze, 1957. Pp. 78.

In December 1956, a group of Polish economists spent several days in Yugoslavia, apparently in order to find out as much as possible about the Yugoslav economic model and its possible application to Polish conditions. Two of the participants have published their impressions in this small volume, the first part of which treats the system as a whole, with the second part concerned exclusively with the problem of workers' councils. The volume was written exclusively for Polish consumption soon after the political upheavel in October 1956.[1]

The author of the first part, Professor Brus, is at present one of the leading spirits in the campaign to liberalize the Polish economy. With minor reservations, he does not hide his admiration for the Yugoslav model, and he devotes by far the largest part of his section to description of the mechanism of the Yugoslav central economic plan. Unlike the detailed plans of the Soviet bloc countries, the Yugoslav plan contains only a set of targets to be eventually realized as a result of economic processes generated by a combination of measures chosen by the planners. The means employed include differential depreciation, interest, and turnover and income tax rates, allocation of profits in enterprises, wage scales and distribution of investment funds. The independent enterprises must then work out their own plans based on these data. Brus emphasizes that the system does not "subject the economy to spontaneous market processes." The plan provides the general framework within which the individual enterprises are free to choose alternatives. The choice is not ab-

[1] For a recent description of the Yugoslav economy see "Economic Planning and Management in Yugoslavia," *U.N. Econ. Bull. for Europe*, Nov. 1958, *10* (3).

solutely free since the system of economic incentives embodied in the plan creates sufficient pressure upon the enterprises to make their plans comply with the central planners' preferences.

It is this lack of rigid centralism and of direct controls that appeals mostly to Brus. He realizes that any socialist economic model must somehow combine central planning with the decentralization and freedom of choice. In his opinion, the Yugoslavs manage to connect both elements in the right proportion, leaving enough freedom to the enterprises to operate on their most efficient level and yet preserving enough control to eliminate any tendency towards monopoly. Enterprises can select their own suppliers, both at home and abroad, and plan their operations on the basis of market demand. Brus is particularly impressed with the foreign trade policy and with the fact that the level of domestic prices is not completely divorced from world prices, as it is in Poland. At the end of 1956, all prices but those for some basic foodstuffs and for important producer goods were apparently the outcome of free-market forces.

Brus provides a useful analysis of the relationship between output, profits and the level of wages. Originally, worker earnings were entirely dependent on profits. This system was soon abandoned because of the great differences in profitability among enterprises and consequently in earnings in the same occupation and industry, and because of the existing inflationary pressures. Since then, wages have been divided in two parts of which only the smaller part is determined by profits. Basic wages are quite rigid, wage differentials are very low and work norms are revised drastically every few months. According to Brus, this system weakens incentives to acquire skills, to increase productivity and to get rid of unproductive workers. One interesting point is the absence of a tendency to underestimate planned output in order to achieve a high rate of overfulfillment and high premiums. Since part of the wage fund is connected with profit, it is the final result that counts, and any overfulfillment of the plan has a relatively insignificant effect on wages. The reverse effect occurs in Poland.

The final part of Brus' volume deals with his appraisal of the Yugoslav model. He tries to answer here the question why, in spite of the measures listed above, relatively little progress has been made in certain respects, particularly in average consumption per head. He explains this in three ways: First, the rapid rate of industrialization, with over one-third of the national income diverted to investment, has had the same negative repercussions as in the Soviet-bloc countries. Second, the international situation until 1955 caused a break-off of commercial relations with the Soviet bloc and forced Yugoslavia to spend on defense up to about 18 per cent of its national income. Third, "the lack of a correct policy in agriculture" apparently compares very unfavorably with the rest of the economy.

The author specifically disagrees with the Yugoslav policy on only two points. He favors fixed prices for producer goods, arguing that violent movements in these prices are bound to affect the prices of final products, thus threatening to destroy the equilibrium in the relatively free market for con-

sumer goods. He also comments on the lack of differentiation between industries of nation-wide and local importance that results in often irrational distribution of investment funds and subsidies.

Brus ends by listing three elements of the Yugoslav system which he would like to see incorporated in the Polish model: the central plan influencing the economy not through direct controls but through economic incentives; real independence of enterprises; and the system of workers' councils. For many years the Poles believed that a planned economy could only operate on the basis of highly centralized controls of the Soviet type, which meant considering the Yugoslav model as a "betrayal of socialism." This opinion, the author concludes, was patently wrong.

The second part of this volume, written by Dr. Jakubowicz, goes over much of the ground covered by Brus, in addition to providing fairly detailed study of the Yugoslav workers' councils. His study does not contain any particularly useful analysis and can properly be classified as a pamphlet glorifying the councils, as was the fashion at the time of its writing.

The section written by Brus can serve as an interesting, although hardly extensive, description of the Yugoslav model, presented in true scholarly fashion. It should contribute to knowledge of Yugoslavia not only in the East but also in the West.

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Business Fluctuations

The Relationship of Prices to Economic Stability and Growth: Compendium of Papers Submitted by Panelists Appearing Before the Joint Economic Committee; Hearings Before the Joint Economic Committee, May 12-22, 1958. Washington: Supt. Docs., 1958. Pp. xiv, 712; v, 427. $2.00; $1.25.

In 1957, the Joint Economic Committee of the Congress decided on "a full-scale investigation of prices and price-making processes in relation to economic stability and growth." This decision resulted in a compendium of papers by 47 leading economists published in May 1958. Later that month, most of the contributors appeared before the Committee for a series of panel discussions of the papers submitted. Despite efforts of the Committee staff to direct contributions over a broader range of topics, the discussion continually adverted to the behavior of the price indices during the 1957-58 recession. In consequence, the papers—and even more so the panel meetings—furnish an interesting cross-section of professional opinion as to the unorthodox interaction of demand and prices during this period.

Without too much simplification, it can be said that four viewpoints emerged. The first held the continuing increase of prices over the past two or three years to be attributable wholly or in the main to forces originating on the demand side of the market. This viewpoint considered the proper weapons for maintenance of price stability to be monetary and fiscal policy, perhaps sharpened by greater experience, perhaps (in the opinion of some mem-

bers of this group) supplemented by more specific credit controls. Most adherents to this view foresaw no serious clash between reasonably defined goals of full employment and stable prices.

The second group, although usually in considerable agreement with the first, felt that the supply response to increased demand was not simply an induced response but also a monopolistic one. This group wanted monetary-fiscal policy reinforced by stiffer enforcement of the Sherman Act, and sounded the call for "more competition" in order to maintain the compatibility of full employment and stable prices.

The third group, although agreeing with the second that supply-side forces demanded attention, questioned explanations framed in terms of simple monopolistic conspiracy. They questioned, that is, explanations drawn from conventional price theory, preferring to build their analysis around some version of the "administered price" concept. Having accorded more autonomy to supply forces, members of this group tended to be more sceptical of the ability of restrictive monetary-fiscal policy to maintain price-level stability without excessive social cost.

The fourth group was most openly hostile towards the Federal Reserve's restrictionist policies. The emphasis here was not on structural or institutional supply-side features, but on the dangers of a near-obsession with a stable price level, in disregard of considerations of equal or greater importance—productivity and growth.

The most forthright analysis of price-level behavior was that of Milton Friedman, who argued from a qualified version of the quantity theory: the dominant influence on the price level is exerted by the stock of money per unit of output. Although conceding minor roles to other factors, Friedman argued that there is a close link between money-stock and price changes in short as well as long periods; and he gave the money stock the dominant role in this relation. But Friedman insisted that there may be a considerable, and unappreciated, lag—over a year—before the full effect of any money-stock change is felt, thus explaining recent deflationary phenomena in terms of the delayed impact of 1956-57 tight money policies. The key to price behavior thus lies in monetary policy—but not discretionary policy. Although in general approving Federal Reserve policy since 1951, "I am myself inclined to believe that in our present state of knowledge and with our present institutions, even this policy has been decidedly inferior to the much simpler policy of keeping the money supply growing at a predesignated rate month in and month out with allowance only for seasonal influences. . . ."

If the money stock exerts its influence over prices by being spent, then presumably this is a Group 1 or demand-side interpretation (although this designation is the reviewer's, not Friedman's). Excess money demand was stressed also by Friedman's colleague, C. F. Christ. But Christ did not advocate nondiscretionary policies; on this point, O. H. Brownlee seemed almost Friedman's only supporter. Herbert Stein concurred with Friedman to the extent of attributing recent price-level rises to a lagged supply response to earlier demand.

Arthur Smithies foresaw serious danger of an unrestrainable price-wage

rise mainly in an overambitious attempt to guarantee full employment; occasional minor recessions, presumably inevitable even with the best of monetary-fiscal policies, would thus furnish the most effective check. The Group 2 contributors, such as Neil H. Jacoby, showed greater concern over demand-originating inflation. Jacoby's anti-inflationary program demanded (in addition to more effective monetary-fiscal policy) vigorous antitrust enforcement, gradual withdrawal of government price-fixing and price-support programs, and lower tariffs. G. W. Ensley, E. G. Nourse and G. L. Bach all stressed the maintenance of competition via antitrust enforcement. But S. N. Whitney, whose paper analyzed the consequences of a series of past antitrust convictions, found little evidence of any significant change in price behavior attributable to a conviction. R. A. Musgrave considered any thought of a return to competition, as an anti-inflationary device, utterly unrealistic.

The most provocative viewpoint offered was probably that of M. J. Bailey, who insisted that administered prices are practically nonexistent in the unregulated sector of the American economy, that the subject is not a proper concern of public policy, and that Congress ought not to waste time in its study. As to the unimportance of administered prices, Bailey had some support from his Chicago colleague, A. E. Rees. Unfortunately, Rees was not present when this support was most needed, at the panel discussion when Senator O'Mahoney, who appeared to regard the Bailey viewpoint with something less than unqualified approval, gave voice on the subject. A later panelist summed up Bailey's argument as "both moderately right and substantially wrong." Part of this argument, sometimes overlooked, was that an administered price is something different from a simple monopolistic price. In this, Bailey was identifying himself with Group 3 rather than Group 2, and he may well have been more than moderately right. But in then adding that monopoly prices are important, administered prices unimportant, he returned to Group 2.

The most comprehensive Group 3 argument was that of Gardner Ackley, who interpreted recent price behavior as a by-product of jockeying for income positions as between labor and capital. Ackley insisted that account must be taken of the administered-price phenomenon, and that "mark-up inflation" is an intellectually respectable form of analysis. Administered prices are to some degree insulated against market demand forces, and to this extent can live a life of their own. Althought he insisted that mark-up inflation is more than simple cost-push inflation, Ackley's argument seems consistent with points made by Ruth P. Mack and subsequently cited in several panel discussions. Mrs. Mack observed that most crude-materials prices still fluctuate freely, but that prices beyond the crude-materials range seem to work on a ratchet. An increase in raw materials prices sends succeeding prices up. But an increasing accumulation of overhead costs relative to direct costs in fabricating industries provokes a fear of price reductions; hence fabricated-good prices do not fall when raw materials prices fall. Strongest support for the Group 3 position (although with differences in emphasis) seemed to come from those who had been involved in statistical or other empirical price-involving work: Mrs. Mack, J. W. Kendrick, B. G. Hickman, R. F. Lanzillotti, and A. R. Oxenfeldt.

No deep conflict between Groups 3 and 4 was necessarily involved, save as to the points most in need of emphasis, and several contributors took intermediate positions. The Group 4 leader was Richard Ruggles, who argued that price indices and unemployment figures are seriously inadequate as all-around measures of performance. Ruggles felt (as did many participants) that the inability of the Consumer Price Index to take full account of quality improvements means that it must overstate the true degree of price increase. The increasing proportion of overhead labor (supervisory and technical) whose employment continues even when output drops means that unemployment figures fail to reveal the full extent of output loss. Despite technological change and expanded capacity, there have been significant increases in output per manhour in only two of the past five years. Denying the argument that a mature capitalist country must submit to a relatively low growth rate, Ruggles argued for increased investment in productive plant and equipment and an emphasis on growth. The much-praised automatic stabilizers choke off rises as well as falls—"we are in fact in the position of being strangled by automatic stabilizers." Continuation of present behavior would mean that "any growth that is achieved must be crowded into a small space of time; all the rest of the time will be recession, recovery from recession, or levelling off prior to going into recession." In answer to those who fear that increased investment demand would add fuel to already-burning inflationary fires, Ruggles pointed out (as did some others) that high productivity is an anti-inflationary counter to the increasing wage demands that may prevail in any circumstances; and high productivity comes when the economy operates close to capacity.

Virtually all contributors paid at least lip-service to the goal of price stability. Some viewed the prospect of secular price increases with alarm. Others, Ruggles included, found it tolerable—not desirable, simply tolerable in comparison with the alternatives to price rise and their consequences. Estimates of tolerable increase seemed to range from 1 to 3 per cent annually. Several Group 3 or 4 economists felt that some form of public administrative body might be needed to deal with wage-price problems. Ackley tentatively suggested a "watchdog commission." A. G. Gruchy proposed a "negotiational-consultative body" along Scandinavian lines. Abba Lerner advocated a body possessing more explicit powers: an agency to furnish administered regulation of administered prices.

An arbitrary squeezing of viewpoints into four pigeonholes may do injustice to individual participants; yet it serves to illustrate agreements and disagreements. As to the broad area of disagreement, two points seem noteworthy: (1) In the main, these are not normative disputes, conflicts as to the proper goal to be pursued. They are disputes as to the way things work. (2) They concern a topic which has been the traditional preoccupation of economists for over a hundred years: price. It would appear that there is analytic work still to be done in the analysis of price determination and price behavior.

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Can Inflation be Controlled? By HAROLD G. MOULTON. Washington: Anderson Kramer Associates, 1958. Pp. xii, 302. $4.95.

The objective of this volume, in the words of its author, president-emeritus of The Brookings Institution, is ". . . a thorough-going reappraisal of the forces responsible for changes in the general level of commodity prices" (p. 8).

Dr. Moulton starts with a review of the state of confusion extant on the subject of prices and money. In spite of the inroads made by the income approach, most textbooks deal with the price level as a monetary phenomenon explained by some version of the quantity theory. Economists, senators, members of the Board of Governors and others serve up explanations of inflation which range through such causal elements as the abandonment of the gold standard, trade unions, government deficits, demand-pull, cost-push, and debt monetization.

Moulton finds that most of these explanations neglect the way in which money comes into existence. Goods and money are integrally related and one cannot increase without the other. The thesis of this volume is that price changes must be analyzed through the forces that increase or decrease the ratio between factor costs and output. Rising costs are the only factor which exerts *pressure* toward higher prices. Prices are set on the basis of costs established before goods come into the market.

The adherents of the demand-pull explanation of inflation, the author finds, are on very weak ground. An examination of the differential behavior of wholesale and retail prices demonstrates that the pressures come first on basic raw materials, and are transmitted by costs, not by demand.

The author finds support for these views in an examination of price behavior in such diverse experiences as the American Revolution, the Civil War, the French Revolution, the period following the first world war in Germany and both world wars in the United States. Further evidence, both analytical and statistical, is set forth in an appendix which makes up almost half the book, where the author examines such matters as the early quantity theory, Irving Fisher, and various attempts at statistical verification of the relationship between the price level and the quantity of money.

Moulton argues that long-term peacetime trends in the price level are shaped primarily by the cost-increasing effect of upward changes in money wages rates and the cost-reducing impact of improvements in productivity. Only if the latter exceed the former can there be price stability. Efforts to balance government budgets, to control interest rates, or influence the quantity of bank reserves will have little or no effect in the absence of a balance between wage rate increases and productivity. The only way to control inflation would be to control wage negotiations, but this would violate forces that ". . . are an inherent part of the private enterprise system" (p. 179). Both rights and human interests are involved, and, it may be inferred, in Moulton's view these are more important than the resulting inflation. The book ends with the hope that labor, management-stockholders, and consumers will all share equitably in the gains of technological progress.

In spite of an over-brief treatment of a great many significant points, this book is a contribution to monetary theory. At a minimum it is a useful offset to those who have recently become enamored with the rediscovery of money.

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Gosudarstvennyi bank SSSR: Kratkii ocherk k sorokaletiiu Oktiabria (The State Bank of the USSR: A Brief Outline on the Fortieth Anniversary of October.) By V. F. POPOV and others. Moscow: Gosfinizdat, 1957. Pp. 254. Rbl. 9.35.

Since the annual flow of Soviet publications on money and banking and related matters is large, interest attaches to this particular volume chiefly because of the circumstances under which it appears. This is, in effect, an official history of the State Bank (the principal short-term lender and principal source of money creation in the Soviet economy). Its authorship is credited to a group of the Bank's employees, writing under the direction of V. F. Popov, chairman of the Bank. As its subtitle indicates, it is published in commemoration of the October Revolution. It appears at a time when secrecy has been relaxed for sectors of the economy which would seem vastly more sensitive than that of banking. All of these circumstances suggest that the publication of this work could have provided the occasion for the release of comprehensive quantitative data on the Bank's operations.

Disappointingly, and with exceptions to be noted, the occasion has not been taken. Rather, like publications in the field for many years past, this work consists almost entirely of institutional and descriptive material: the organizational structure of the Bank, its lending procedures, modes of rendering payments, currency circulation (the chapter on this subject contains most of what is said on the Bank's planning and control functions, which are otherwise little emphasized), the Bank's operations as fiscal agent of the budget, its handling of international settlements, accounting and operating procedures, etc. Moreover, while the institutional information provided here is handily arranged either for a brief introduction to Soviet banking practices or for easy reference by the specialist, it is generally available in more extended discussions in other sources.

With respect to quantitative data, it should be first understood that detailed balance sheets for the Bank ceased to be published in 1932, complete but highly consolidated accounts were published from 1933 to 1937, scattered fragments were released until the start of the war, after which scarcely a single absolute figure was released until figures for total loans again appeared in the early 1950's. Hence the anticipations which this book arouses and largely—though not quite entirely—disappoints. There are some new figures here, principally on pages 58-59 and in a statistical appendix.

Most revealing are several postwar figures for deposits held at the Bank by

enterprises; these are so small as to suggest that the enterprise sector has remained as chronically illiquid as it was before the war. Figures on deposits of collective farms reflect the post-Stalin rise in farm incomes; a single figure for deposits of individuals indicates that these remain insignificantly small. On the critical issue of the size of the Budget's deposit holdings, which are known to be very large, the source reports merely that balances of "the financial-credit system" in 1957 were 11.2 times the prewar level (when loans were 4.4 times the 1941 level). No new data on currency emission are given here, although the Minister of Finance has felt free to report elsewhere, also in 1957, that currency at the end of the war was 4 times the prewar level and by mid-1949, after the 1947 conversion, was only 30-40 per cent above prewar. Also on the liability side, useful figures are given for what are essentially items in transit. On the asset side, loans are here shown to have increased by little more than 8 per cent from 1941 to 1946, which raises again a long-standing question of the source of the wartime currency expansion, beyond that which can be accounted for by budget deficits. Extremely detailed data on the distribution of the Bank's loans by purpose and by sector suggest likely explanations of recent fluctuations in the volume of loans, including a 22 per cent increase in 1956 alone, and indicate clearly that the much-heralded changes in lending procedures decreed in August 1954 have had little quantitative impact. Nothing is said of reserves, capital accounts, or total assets, and no hint is given on the handling in the Bank's accounts of the 1947 conversion or of the inventory gains and losses known to have been shifted to the Bank as price levels changed.

In general, the amount of new information given here is large relative to that previously released but small relative to that required for a reasonably complete understanding of the Bank's operations. It is enough to be tantalizing but too little to be satisfying. Perhaps one should, nevertheless, be thankful for small blessings.

One other deficiency of the volume merits comment: its almost total lack, except for some obscure implicit theorizing in the chapter on currency circulation, of anything which could be called analysis. A few years back, this would have been in no way noteworthy. But from some time after Stalin's death, Soviet writers on monetary matters began to raise the kinds of questions which present themselves immediately to the Western observer: What criteria justify the present division of responsibilities, for financing the economy, between the Bank and the Budget? What function is served by the budgetary surpluses accumulated as deposits at the Bank? (One reviewer of a textbook published in 1956 complimented its author for merely raising the latter question, though surpluses have been run since 1924 and large surpluses since 1933). What accounted for the inflation that persisted into the late 1940's? The answers offered have been generally faltering and naïve, but the reawakening of a spirit of inquiry which they indicate is intriguing and even heartwarming to watch. That no such spirit is evident in the book at hand may be no more than a reflection of its official character.

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Public Finance

Classics in the Theory of Public Finance. International Economic Papers No. 8. Edited by R. A. Musgrave and A. T. Peacock. New York and London: Macmillan, 1958. Pp. xix, 244. $6.00.

This volume is a collection of late 19th—early 20th century contributions to the classical problem of public resource use in a fully employed economy, which has recently been regaining a central position in public-finance theory and policy after a long period of displacement by the problem of stabilizing effective demand. All sixteen selections (except one from Edgeworth included because of special relevance) are original translations from works of continental European writers: ten from the German (including work of the Swedes Wicksell and Lindahl), four from the Italian, and one each from the French and Dutch. The selections are arranged chronologically beginning with extracts by Wagner published in 1883 and terminating with excerpts by Ritschl published in 1931. A 10-page introduction by the editors very helpfully identifies and traces the main trends of thought running through the various contributions and relates them to the concurrent development of general economic thought.

The most interesting and perhaps most significant aspect of the *Classics* is that many of the issues of methodology and policy which concerned the contributors are familiar as components of recent discussion. (Cf., for example, Gerhard Colm, *Review of Economics and Statistics,* Nov. 1956; Julius Margolis and R. A. Samuelson, same journal, Nov. 1955; J. K. Galbraith, *The Affluent Society,* Ch. 22 and passim.) Some of these issues and the treatment they received one-quarter to three-quarters of a century ago are, in brief, as follows:

1. What is the comparative productivity of normative and positive theories, and how construct the latter? Most of the authors in the main do take a normative approach. Exceptions are Wagner in his explanation of the growth of public expenditures, Stein in his historical treatment of taxation, and Goldscheid who vigorously claims public finance for sociology and criticizes the cart-before-the-horse approach of setting policy norms prior to understanding the function of public finance in state and society.

2. Is the indivisibility of public goods—that is, providing for one provides equally for all—a centrally important feature? In general, yes.

3. Are individual preferences the main criterion for allocating resources to the public sector? A resounding "No!" from Ritschl, for whom the state serves communal needs subjectively felt by competent authorities and by individuals who spiritually identify with the community; others must be coerced. Montemartini considers the product of government to be not service at all but coercion. Barone clearly rejects individual valuation as a basis for taxation. Edgeworth and Cohen Stuart, exemplifying the ability-to-pay approach, split off tax policy from expenditures and attend exclusively to the former, elucidating via marginal utility analysis the implications of minimum, equal and equal-

proportional sacrifice for tax-rate progression. Others, by implication affirmative, are discussed in (4).

4. Can the quantity of public goods and their cost distribution be such as to permit an optimum want satisfaction comparable to that for private goods and is there a workable means of attainment? Mazzola, having early introduced the indivisibility feature, argues that quantity consumed not being individually variable, correspondence between marginal utility and price requires differential prices (taxation), which he concluded (wrongly) would eliminate consumer surplus. Whether the equilibrium force could overcome disturbances from the political sphere is unclear. Sax, restating a forty-year-old position, accepts that taxes should vary with the money-worth of public goods to the individual (whose initial valuation, however, necessitates a spiritual bond with the community).

In a long and wise essay, Wicksell, responding to Mazzola and Sax renders an all-right-in-theory-but-not-in-practice verdict. The individual, whose benefit depends on the payments of others and insignificantly on his own, would bid nothing for public goods. Therefore he suggests that all proposals be submitted in an expenditure-*cum*-tax form and be adopted only by unanimous or approximately unanimous vote.

Lindahl in a rigorous analysis views the evaluation of public goods as analogous to private pricing of joint products, the supply schedule of government services to one taxpayer-demander depending on their total cost and the proportion covered by others. At equilibrium, the level of expenditure and its cost distribution is determined so that for each taxpayer the marginal gain equals the marginal cost. The analysis, initially developed in the context of a community of two taxpayers possessing equal political power but unequal income, is considered extensible to large numbers and in principle valid for cases of unequal power (but cf. Musgrave, *Quarterly Journal of Economics* Feb. 1939, and Samuelson, *op. cit.*). Individual preferences become effective through the vote and the fact that fiscal decision-makers always take the viewpoint of some average citizen.

5. Should taxation be used for wealth redistribution independently of cost distribution? Edgeworth, yes, but not to the income leveling extreme implied by the minimum sacrifice doctrine; Wagner, yes, as part of the social welfare concept; Wicksell, a cautious yes; Lindahl, yes for central but not for local government. Wieser, never.

Aside from speculation on the form and substance of intellectual progress, the primary reaction to the book is one of gratitude to the editors and the Association for making these works available for what is probably the first time to most economists in the field—including the reviewer. The teacher can certainly derive much utility from its use as a source of supplementary reading, particularly if he sees pedagogical merit in a development-of-thought approach to the subject.

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Public Finance. Edited by R. W. Lindholm. New York: Pitman Publishing Corp., 1959. Pp. xviii, 798. $7.25.

This book was written by the Committee on Public Finance which consists of 66 professors of public finance from all over the nation and from all types of educational institutions. An attempt was made to write a definitive textbook in public finance, or certainly one that might be preferred by a majority of teachers in that field. While the committee method is not usually considered the ideal method by which to write the ideal textbook, the Committee on Public Finance has been very successful in integrating the material and presenting it with clarity, precision, and a common style. Undoubtedly the Committee benefited from the experience and criticisms of the earlier Pitman book, *Money and Banking* (1957), written by a 59-man Committee on Money and Banking, and reviewed in the December 1958 issue of the *American Economic Review*.

The emphasis of this book is on balance and comprehensive coverage rather than on controversy. Theoretical analysis is combined in proper proportion with the practical realities of the subject matter. Yet none of the recent theoretical developments in the field of public finance and fiscal policy is omitted or overlooked.

As for the earlier book, separate committees were established to plan and to write each chapter, but each chapter benefited from the mutual criticism and cooperation of other members and committees of the project as well as from the editorial staff. While the quality of the individual chapters varies somewhat, the over-all result is a rather constructive and challenging piece of work.

The book is divided into the following parts: Introduction, Expenditures, Taxes and Revenues, Public Debt, Fiscal Administration, and Fiscal Policy. The Introduction discusses the financing of the public as contrasted with the private sector of the economy, and the role of the government in a market-oriented economy. Part II devotes 10 chapters to all of the major federal expenditure programs as well as to the general patterns of public expenditures. Part III in 9 chapters covers all the important federal, state, and local tax sources as well as the criteria for allocating taxes by level of government and the subject of shifting and incidence of taxation. Part IV covers the relationship of the public debt to the money supply and also the economic effects of public borrowing. Part V devotes 4 chapters to the formulation, administration, and execution of the federal budget, the planning and control of public expenditures, state and local administration, and intergovernmental fiscal relations and problems. The last part in 5 chapters discusses fiscal, expenditure, tax, and debt policy, and also the basic patterns and historical evolution and application of fiscal policy.

All in all, the book is thorough, complete, and readable. The data are current generally through 1956 and 1957. The discussion questions at the end of each chapter as a whole are good.

The length of the book is not particularly objectionable because parts can

if necessary be omitted in the conventional introductory course in public finance. Then, too, sufficient material is presented for certain follow-up courses in that area. However, a good bibliography at the end of the book, or better still, at the end of each chapter or part of the book, would have been of distinct advantage to the teacher and especially to the student.

The book has a number of novel features, only a few of which can be mentioned here. For example, Chapter 2 has an excellent discussion of the determination of governmental powers and responsibilities, and the distribution of these by level of government; Chapter 21, of the desirable size for the money stock or supply as it relates to fiscal policy and debt-management problems and objectives; Chapter 26, of the direct, indirect, and implied federal limitations on the power of state governments to tax. Chapter 29 has a unique discussion on the formulation of tax policy for each level of government.

Needless to state, with 66 co-authors, the book will have many adoptions. Apart from that fact however, the book warrants the attention of all serious students of public finance and fiscal policy.

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International Economics

Globe and Hemisphere: Latin America's Place in the Postwar Foreign Relations of the United States. By J. FRED RIPPY. Chicago: Henry Regnery Co. in coop. with Foundation for Foreign Affairs, 1958. Pp. xi, 276. $6.00.

This is a topical book on what to do about Latin America by a veteran student of the area. More exactly, it is a series of brief historical essays that, as happens almost always, shows the historian to be more interested in prescribing for the future than recording the past. The "lessons of history," of course, are what the historians would teach us, history or no history, but it's a pity they do not meet issues head-on, working openly in the light of reason, weighing arguments pro and con. The direct approach might at least provoke a good debate that unfortunately is not encouraged by any number of asides strewn among the interstices of a loosely constructed series of historical narratives.

It is not to be denied, however, that there is some interest in reading the various episodes in recent inter-American relations that make up the bulk of this book. Its 14 essays include three on past U.S. investments in Latin America; two of general scope on our economic aid to these countries; two on specific programs of assistance (the inter-American Highway and the canal zone projects in Panama and Nicaragua); one on our massive aid program in Bolivia; separate chapters on the problems of rubber, sugar, and fisheries; an introductory essay on the Western Hemisphere concept; one on cultural relations; and a final one entitled "myopic drifting" that returns to the theme

of foreign aid along with an obiter dictum on how such aid allegedly fosters statism in the receiving countries. There's something for almost all tastes, written in an easy style, and rich in documentation and tabular material. Very often a healthy muckraking spirit enlivens proceedings, especially in the episodes of Nicaragua and the "rubber-planting fiascos in tropical America." In fact, the author shines in narrating little-known fiascos, but his foreign investment chapters, which would ordinarily be of most interest to economists, do little more than repeat an oft' told tale. He would also be more convincing were he less *ad hominem* and more *ad rem*, treating proposals on their merits, not on the supposed motives of their sponsors.

The author's main point is that we should keep a place for hemispheric commitments within our global ones. At the outset he announces his opposition to expending "more energy and larger resources in shaping the destiny of the peoples of the Old World than in improving the fortunes of the New" (p. 1). To the obvious objection that our national security lies in alliance with the Old World, he answers that "Maybe the defense of Europe can soon be left largely to the most efficient and dependable European nations with their intermediate-range missiles. Then the United States could gradually resume its traditional policy of emphasis on the Western Hemisphere . . ." (p. 239). But maybe not? Surely our foreign policy should not rest on the success of one kind of missile. More importantly, he believes that the Western Hemisphere concept preserves a military basis: "But the Western Hemisphere still serves as the 'inner fortress.' Though more exposed and less impregnable than it used to be, it is still the inner fortress" (p. 28). Also: "And let it never be forgotten that this Western Hemisphere, where our ancestors had a chance to make a new start, is our inner fortress and the only base of operation we have; that all would be lost if it were weakened beyond repair" (p. 200). Of course, all this is very doubtful, but it's a matter more for generals than economists.

Rippy would support foreign aid only if it were tied exclusively to our national security. Hence on aid programs: "The programs should be restricted to countries of supreme importance to national security, which should be their prime, if not their sole, motivation" (p. 230). Again: "Only such expenditures as are strictly involved in safeguarding the security of the United States can truly be described as outlays serving the national interest" (p. 238). But this is not to be taken as a plea to multiply aid to the countries of our so-called inner fortress: "The conclusion that [what] the hemispheric neighbors received in fiscal years 1956-57 was about as much as they deserved in the circumstances appears to be justified" (p. 233). The correct policy would therefore seem to be a drastic cut in foreign aid whose allegedly heavy burden on the taxpayer is indeed deplored several times. Such a line of reasoning would also suggest that Rippy is one of the last of the isolationists; in a short review one cannot say more than more power to him in challenging accepted notions, but his isolated salvos in fact carry little firepower.

The author is aware that Pan-Americanism means more than military security based on propinquity. He writes that "Yet the Western-Hemisphere

concept in its full flower seems to have embodied ample common convictions, ideals, and objectives to bind the Americas together in a unique practical relationship. Perhaps the concept was always partly fictitious; but faith and trust have a way of transforming fiction into fact" (p. 199). So wishing does make it so, after all. Or does it? Who still believes, looking south as well as north, that the New World is the home of free government and the Old World the mother of despotism? If common political systems were a necessary basis for alliances, which is belied by our special relation to Spain and Yugoslavia, then our most intimate ties would be with Western Europe, not Latin America. Too much is often made of a community of ideals in the Western Hemisphere when reality falls short of ideals, by yards in the one case, miles in the other. Meanwhile there is a movement under way (in the talk about a "common market," for example) to bring together more closely the twenty Latin American countries. This movement has some roots in the past, and it will be interesting to see how it prospers in the face of growing American ties across the Atlantic.

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Financing Free World Trade with the Sino-Soviet Bloc. By RAYMOND F. MIKESELL and JACK N. BEHRMAN. Princeton: Princeton University Press, 1958. Pp. viii, 109. 25¢. No charge outside U.S.A.

This small volume is the latest in the series of paper-bound Princeton Studies in International Finance. While as the title indicates it is concerned with the methods by which the "free world" and the Sino-Soviet bloc have financed their mutual trade, much of the book deals with the theory and practice of bilateral agreements, since this type of agreement has predominated in East-West trade. In 1955, for example, 71 per cent of free world-bloc trade occurred between countries with bilateral agreements; in 1957, there were roughly 240 bilateral trade or payments agreements between these two groups of nations.

What has motivated such a large amount of bilateralism? In the case of Western European countries, the bilateral agreements are largely a residue of severe currency problems of the earlier postwar period and are declining in number. The less-developed "free world" nations have a variety of motives. Bilateral agreements with the bloc provide them with opportunities to: dispose of temporary surpluses of primary commodities; sell in more stable (both as to price and quantity) primary-product markets; obtain credits for import surpluses as well as technical and financial assistance; sell exports despite overvalued exchange rates. On the other side of the fence, bloc motivation is both political and economic. Commercial advantage has clearly been dominant in the expansion of bloc trade with Western Europe—but this expansion has become increasingly "multilateral." In the case of bloc trade with the less-developed nations, there are also strong economic factors: the need for raw materials; and the opportunity in many cases to obtain these materials in exchange for manufactured goods which might not be acceptable in more de-

veloped countries at world prices. Bilateral agreements also provide the bloc nations with greater bargaining power in dealing with these "weaker" trading partners and also tend to orient the latters' trade toward the bloc. The usefulness of bilateral trade and payment agreements for exerting political pressures is, of course, obvious. While the Sino-Soviet countries have not always, nor perhaps often, conceived their bilateral trade agreements with the political factor in mind, they have used them to some political advantage in a number of instances documented by the authors. Special cases of this are the series of long-term credits to less-developed nations negotiated in the bilateral framework since 1955.

Much of the raw material upon which this book is based was gleaned by the authors from the unclassified files of the Department of Commerce and from information provided by the State Department regarding bloc trade agreements with other nations. The bulk of the book is devoted to analyzing and classifying these data in order to reconstruct the salient characteristics of bloc-free world trade. Among other things, the authors cover payments agreements, commercial and banking practices, agencies conducting trade, pricing practices, trade and commercial practices, and so forth. Their analysis is competent and interesting. Of especial interest was the presentation of case studies to illustrate generalizations.

One generalization which particularly struck me, having just recently read some Russian materials on the advantages of Soviet-type bilateral trade, was that bloc bilateral trade has been both (1) subject to wider fluctuations than unplanned trade among nonbloc nations and (2) wide of targets regarding the value of trade. Soviet literature leaves the opposite impression on both counts.

This is a useful, interesting and (in my opinion) unprejudiced empirical study in a field in which economists and politicians are too prone to prejudiced deductive generalizations. It should be read by everyone concerned with East-West trade. It contains, incidentally, in addition to the officially listed 109 pages of text, a 145-page unpaginated appendix summarizing the agreements of Soviet-bloc countries with the West.

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Soviet Economic Aid: The New Aid and Trade Policy in Underdeveloped Countries. By JOSEPH S. BERLINER. New York: F. A. Praeger, Inc. for Council on Foreign Relations, 1958. Pp. xv, 232. $4.25.

A descriptive analysis of what goes on behind an iron curtain of secrecy and propaganda is no easy task. And a study which by its nature requires a good deal of conjecture, estimation, and guesswork is not without frustration. Yet the challenge of such an endeavor must have been attractive enough to invite Professor Berliner to write the story of Soviet aid to underdeveloped countries.

The book deals with Soviet "aid and trade" programs in the Middle East,

South and Southeast Asia, Africa, and South America in the four-year period between 1953 and 1957. The first half of the study analyzes the rationale of the Soviet program and the nature, type, and relative magnitude of the Soviet and the Communist bloc commitments. The remainder of the book is concerned with an evaluation of the program's benefits to the Soviet economy, the advantageous position of the Soviet Union in this new game of diplomacy, and its capability to continue its aid program. Several charts and tables provide supporting data on bloc credit and trade agreements with selected underdeveloped countries. The bibliography presents a fairly comprehensive list of the major works on the the subject. Notable exception: Klaus Knorr's brief but pioneering study, *The Ruble Diplomacy.*

According to Berliner the Soviet aid and trade program began in 1953 as a result of a major tactical shift in Soviet foreign policy. Until Stalin's death, Soviet policy supported local Communist parties in their opposition to the emerging "bourgeois" governments. Western economic aid was denounced as a capitalist-imperialist device to exploit the underdeveloped areas. The post-Stalin leaders of the USSR, however, discontinued this policy and decided to support "bourgeois nationalists" in their quest for rapid economic development, political independence and military neutrality. Underlying this change in tactics was an effort to influence foreign policies of the newly independent countries of Asia and Africa in favor of the Soviet Union.

Communist bloc aid to undeveloped countries takes a variety of forms: (1) the provision of medium- or long-term (5-20 year), low interest (2-2½ per cent loans and credits to finance capital-goods imports; (2) technical assistance in building and installation of industrial plants; and (3) training of local technicians and skilled workers for the operation of industrial projects. Since, however, the latter types of assistance are normally paid for by the recipients, Communist aid is in effect favorable business loans that ought to be repaid at a later date. In this respect it differs materially from foreign aid offered to the underdeveloped world by the United States.

The value of Soviet bloc credit agreements with 16 underdeveloped countries between 1953 and 1957 is estimated at $1,581 million to be drawn upon over a "number of years." Of this total only $342 million is estimated to have been actually delivered between 1953 and 1957. The total Soviet "deliveries," therefore, hardly measures up to the $2,597 million of U.S. grants-in-aid and credits actually given to the same countries between 1945 and 1957. Compared with the total U.S. aid and loans to the entire underdeveloped world of some $12.8 billion in the same period, Soviet aid ranks even lower. Three-quarters of the $1,581 million credit is extended by the Soviet Union alone, and 90 per cent of it is concentrated in five countries: Yugoslavia, the United Arab Republic, India, Afghanistan, and Indonesia. The magnitude of the bilateral technical assistance agreements between the bloc and aided countries is estimated to be about $6.5 million or roughly the same amount contributed by them to the United Nations' expanded technical assistance programs. This is to be compared with the U.S. government pledge of $72 million to the

same United Nations agency. The total value of gifts offered by the bloc is believed to be under $20 million.

The Communist bloc members consider commercial trade as an integral part of their policy of economic aid. Thus, the extension of long-term credits is generally accompanied by barter trade agreements. As a result, bloc trade with its underdeveloped partners is believed to have risen by more than two and a half times between 1953 and 1955. Despite this remarkable increase, the volume of commercial trade in 1955 was still below its 1948 level in real terms, and lagging far behind the trade of the same underdeveloped countries with the free world. Interestingly enough, although the USSR has provided the bulk of trade credits, the lion's share of the expanded commerce has thus far accrued to the East European satellites and not to the Soviet Union itself. The trade of Soviet Union with all non-Communist underdeveloped countries in 1955-56 accounted for only 3-6% of total Soviet foreign trade—a phenomenon which is indicative of Russia's continued fear of dependence on nonbloc nations.

Economically, Russia gains from trading certain kinds of manufactured commodities (in which it has comparative cost advantage) for certain types of primary materials which it needs. These advantages are now greater than they were in the early part of the century for two basic reasons: (1) a change in relative cost ratios which has occurred as a result of Soviet industrialization in the last 30 years, and (2) the possibility of providing many outlying Soviet regions with imports from neighboring countries at lower transportation costs. These economic benefits, however, are only a small part of Soviet gains. The real Soviet gains from its aid and trade program should be found in its "spreading good will" throughout the underdeveloped world and its increasing direct or indirect political influence in foreign policies of the underdeveloped countries.

Why has the Soviet Union succeeded in gaining such good will and influence with so little material sacrifice? Berliner suggests a variety of political, strategic, and emotional factors. Economics, again, plays a minor role. The advantage of "novelty" is all-important. The West has no longer a monopoly in foreign assistance. Underdeveloped countries can now play Russia against the United States. Anticolonial and anticapitalist biases in the socio-religious traditions of Asian and African people, too, favor closer affinity with Russia than with the West. Attractive terms, absence of apparent political strings, barter deals, acceptance of local currencies and commodities, and the administrative flexibility of the Soviet aid machinery are other points in Russia's favor which the United States government should seriously consider in its aid program.

The author considers Soviet capacity to extend economic aid to be fairly large, although he does not expect Soviet aid to rise substantially in the years to come. Soviet exports to all underdeveloped countries in 1956 amounted to only 1/20 of 1 per cent of its gross national product. Even if the Soviet government were to give underdeveloped countries as much economic aid as does

the United States, it would still spend not much more than ½ of 1 per cent of its 1956 national product. Assuming an annual increase of 5 per cent in Russian productivity, an aid program as large as that of the United States today would absorb slightly more than 10% of the additional annual production. This point, however, should not be overemphasized. Should the aid program be concentrated, as it very well may, on the export of machinery and particularly metallurgical equipment, an export program of even modest magnitude will make heavy inroads into Soviet production at least in the short run.

Soviet Economic Aid is written in a lucid and readable fashion. Combining objectivity of treatment and technical sophistication with informality of style, the study is of interest not only to professional students of Soviet-American economic rivalry, but also to all intelligent readers who are concerned with this new aspect of the cold war. The real contribution of this study to the field of economic diplomacy, however, rests to a large extent on the manner in which the author delves into the various facets of his inquiry, narrows the range of possible alternatives, chooses likely hypotheses, tests them against available facts and statistics, and reaches some important logical conclusions.

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Business Finance; Investment and Security Markets; Insurance

Principles of Finance. By Carl A. Dauten and Merle T. Welshans. Cincinnati: South-Western Publishing Co., 1958. Pp. xii, 596. $6.50.

The authors intend this text for a one-quarter or one-semester survey course in finance, covering the finance functions and the principles, institutions, instruments and procedures that have been developed to carry them out. Presumably it is aimed at those in schools of business administration who have found the traditional course in money and banking unsuitable as a basic requirement. The book is divided into six parts. The first four are devoted to financing in the private sector of the economy, covering short- and long-term sources of funds for business, special areas (agriculture and international finance), and short- and long-term sources of funds for the consumer. The latter two parts are concerned with the monetary system, including the impact of Federal Reserve and Treasury actions and with interrelations between the money supply, prices and production. The text is essentially descriptive in nature.

There is no question that in coverage this work is very complete and will provide the student with a comprehensive picture of the field of finance. The presentation is clear and straightforward; and some chapters, particularly those on merchandising and facilitating agencies for long-term financing, consumer financing, and Treasury powers affecting the money supply, are very good indeed.

However, in a survey book of this type, emphasizing breadth rather than depth of coverage, two very important considerations are (1) organization and (2) selection and emphasis of materials. It is in these areas that the reviewer must express some reservations. On the question of organization, setting up the business enterprise, the consumer, the farmer and the foreign trader as the focus of dicussion may have certain advantages. It certainly facilitates the presentation of descriptive material, but it practically eliminates any well-rounded analysis of portfolio management in our major financial institutions. As one illustration, in the case of the commercial bank, the instructor will nowhere find mention of the role played by secondary reserves in maintaining bank liquidity. The latter is discussed in less than a sentence (p. 485).

Just as serious are imbalances in selection and emphasis. For example, commercial finance companies and factors receive an inordinate amount of space in relation to their minor roles as sources of business credit; much more space is given to types of insurance policies than to the application of insurance company funds (Ch. 11); there is a minimum allocation of space to open-market operations, given their importance as an instrument of credit control (Ch. 23); the stress in "Financing Agriculture" (Ch. 14) is apt to give the student a false impression of the importance of governmental sources of credit in this field. Further, though there is considerable discussion of the income and financial status of households using consumer credit, there is no analysis of differences in the terms and availability of credit to large, medium and small business enterprises.

In sum, a form of organization allowing greater range for analysis, and a more judicious emphasis are required to give the student a proper perspective of the American financial complex. There is still room for a really adequate survey text in the field of financial institutions.

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Basic Business Finance—Text and Cases. By PEARSON HUNT, CHARLES M. WILLIAMS, and GORDON DONALDSON. Homewood, Ill.: Richard D. Irwin, Inc., 1958. Pp. xi, 911. $7.00.

Modern medical schools no longer train students of medicine without anatomy laboratories, clinical facilities, and hospital internships. Likewise, most law schools of high rank utilize legal aid practice, editorial assignments in law journals, and court decisions to educate law students. In strange contrast, schools of business administration generally are not clinically oriented. After more than thirty-five years of development, the case method as a means for educating businessmen remains highly controversial. Only a single business school in the United States has the faculty and other resources to employ case material comprehensively. Few other business school faculties have been able even to agree on the effectiveness of the case method as an educational procedure.

That progress is being made is shown by the increasing number of case-

books in business administration being published. Early casebooks in finance were published by Masson and Stratton in 1935 and 1938. Not much followed until Hunt and Williams published *Case Problems in Finance* in 1949. This book has passed through two editions and five printings. Still in use, it serves different levels of collegiate sophistication from the first course in finance to graduate and executive development courses. Despite this flexibility, a number of professors have wanted cases of less difficulty. Then, too, a need has continued to exist for textual material more closely coordinated with the topical outline of the cases. Hunt, Williams, and Donaldson have responded with *Basic Business Finance—Text and Cases.*

Of the 893 pages of this big book, 581 comprising 27 chapters are devoted to text material and 312 to case material. There are 40 cases, of which the shortest is only two pages; yet it poses for students probably the most difficult challenge of all the cases, a capital planning or investment problem. The longest case, 13 pages, requires a dividend payment decision based on analysis of extensive financial data. Ten cases are reprinted from the preceding *Case Problems in Finance.* However, three are revisions and thus constitute different cases.

After an introductory chapter, the book embraces: (1) the nature of the need for funds (4 chapters, 4 cases), (2) techniques for analyzing past and current financing (1 chapter, 4 cases), (3) forecasting future needs (1 chapter, 4 cases), (4) how funds can best be obtained: short-, intermediate-, and long-term (8 chapters, 14 cases), (5) capital budgeting (2 chapters, 2 cases), (6) markets for corporate securities (4 chapters, 3 cases), (7) government regulation (1 chapter, 1 case), and (8) recurring and nonrecurring financial problems, e.g., dividend policy, financial difficulties, promotion (5 chapters, 8 cases). The point of view is that of the chief financial officer of an operating company. Primary emphasis is upon financial decision-making and administration in a going concern. Secondary consideration is given to the interests of the security analyst, private investor, and trade creditor.

By now enough water has gone over the ideological dam to permit dispensing with a discussion of the relative merits of the internal operating point of view vis-à-vis the social point of view dominant in the traditional book on corporation finance. Suffice it to say that social aspects are not completely ignored. The issues are in the cases even though subservient to operational procedures. They can be brought out to an extent depending upon the interest and understanding of individual professors.

Substantively, the text material advocates the so-called funds approach. Concepts, generalizations, and procedures are related to discovering sources and to planning the uses of funds. The substantive material also supports the analysis of cases. Useful background information includes aggregative data and descriptions of financial institutions. At a more sophisticated level the discussion presents basic ideas on important financial issues. Analytically, the cases are more than illustrations for assertions made in the text. Some require as a first analytical step identification of problem areas, itself a sticky task.

Others state the problem, but require a solution. Still others pose problems but demand the clarification of alternatives and a choice leading to a decision.

Primarily the audience for this book will be students coming to the first course in finance, but they should have a basic familiarity with accounting concepts and statements. *Basic Business Finance* has sufficient substance to make it adaptable for more advanced courses. However, a coming revision of *Case Problems in Finance* with cases to replace those pre-empted by this new book is expected to serve the needs of students at levels beyond a first course.

Instructors who are largely subject-matter oriented will discover that the text compares favorably from a topical standpoint with such yardsticks as Dewing and Guthman and Dougall. This is not to say that the discussion of individual topics is as exhaustive. But it is not the purpose of business finance to imitate corporation finance.

The text is outstanding for its treatment of short-term financing—discovering needs, locating sources, bargaining for workable terms and negotiating contracts. It is equally competent in discussing problems of risk, income, and control in relation to corporate securities. In addition the text remedies an early shortcoming of business finance in emphasizing short-term at the expense of longer-term problems. Firmly encompassed in this version of business finance are such traditional topics as the legal pattern of business organizations, basic security types, security markets, government regulation, mergers, dividend policy, refunding, recapitalization, and business failure. The "something new" is represented by chapters on appraising alternative investments, and cost of capital.

This book is not entirely free of difficulties. Discussion of dividend policy would become more meaningful if it were recognized that this policy is a by-product of asset management and its twin objectives of liquidity and investment. Limiting the chapter on mergers to financial implications closes out the whole history and importance of antimonopoly policy as an area in which the business citizen must live. The appraising of material dealing with investment opportunities and the cost of capital requires, especially for beginning students, a more carefully presented sequence of ideas and clarification of mathematical procedures. The attempt to wed earnings coverage to outlay costs of capital confuses most students and may not convince some professors.

Nonetheless, this comprehensive combination of text and cases makes possible the building of a highly respectable beginning course in finance. There is enough material to support a full year's work including, probably, cases for examination purposes. The cases are not so unreasonably difficult as to defeat students at the beginning. Nor are they simple little examples to demonstrate ideas developed in the text. Average students will make progress with the key issues. Mature students will go beyond to the secondary and more subtle problems in the cases. The strength of this book lies in its essential rightness for training students of business administration.

KARL A. BOEDECKER

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Business Organization; Managerial Economics; Marketing; Accounting

Organized Business in France. By HENRY W. EHRMANN. Princeton: Princeton University Press, 1957. Pp. xx, 514. $7.50.

Professor Ehrmann, known to most of us as the author of a distinguished book on the French labor movement, has now published a companion volume on *Organized Business in France.* The first of its kind, it will be looked to with great expectations both by the economist who wants to understand France's puzzling economic development, and by the political scientist interested in the nature and structure of political pressure groups on the other side of the Atlantic. Neither of them will be disappointed; although each may find in this book more information on his subject than he needs and wanted to have. But this shortcoming may well be inevitable in a book catering to such divergent interests.

The first half of the book is taken up with a 100-page account of the history of business organization from Popular Front days to the Liberation and with a 170-page discussion of its present-day structure. This latter part deals, among other things, with the relations between the different types of business organizations, between the different levels within the same hierarchy of business organizations, and between the bureaucracy of business organizations and their member firms. It also deals with the policies and methods of action of business organizations as political pressure groups; and it analyzes the social origins, educational background, political history, personality, philosophy, and position of all the important leaders of organized business. The detailed discussion of important and not-so-important events and their background, of important personalities and the gray eminences behind the scenes must make fascinating reading to a political scientist; they fill one with awe and admiration for the author's wide knowledge and thoroughness; but the economist is likely on occasion to lose the thread and his patience and to bog down in the welter of names and detail. One is tempted to blame the author for giving quite so much detail and for not organizing and summarizing it better; although it is difficult to offer constructive suggestions for better organization, and much of the detail may well be necessary not only for the political scientist but for the economist as well. For the French economy is so complex and so fraught with contradictions that its understanding requires much more knowledge of this sort than does that of, say, the British economy. The student of the French economic scene must accept the difficulty of his task even if he groans under the burden.

More easily digestible and more interesting for the economist is the second half of the book, whose over 200 pages deal with the attitude of organized business to various economic issues, such as taxation, productivity, nationalization, cartels, European integration, labor unions, worker participation in business management, to mention just the more important ones. At the same time, the reader also picks up much incidental information about the attitude

to these issues of governments, politicians, political parties, and businessmen, big and small; and one is again impressed by the author's detailed knowledge and ability to handle a vast quantity of material. For example, the 20 pages devoted to the issue of economic integration give a well-documented account of the views expressed by the many officials, committees and unofficial spokesmen of different business organizations at various levels and in various industries in connection with the parliamentary and public discussions of some of the early plans for economic integration, of the European Defense Community, of the abortive Franco-Italian Customs Union, of the Coal and Steel Community, and of the European Community in general. Readers should have no difficulty in drawing their own conclusions from all this material but some may object to having to do it themselves. Many economists, I suspect, will turn to this volume in the hope of finding authoritative statements on where French business stands on a variety of issues. What they will find instead is the many tidbits of factual information that form the raw material out of which authoritative opinions are made.

TIBOR SCITOVSKY

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Industrial Organization; Government and Business; Industry Studies

The Structure of British Industry: A Symposium. Edited by DUNCAN BURN. New York: Cambridge University Press, 1958. Vol. I, pp. xvii, 403; $8.50. Vol. II, pp. xiii, 499; $9.50.

When the definitive history of the doings of economists during the twentieth century is written it will remark on the rise in Great Britain during the period 1935-60 of a considerable number of economists who concerned themselves with, and were extremely knowledgeable about, the structures and workings of British industries in their setting in the total British economy. It will be noted that both the surge in interest and the transformation in approach were dramatic. One of the impressive items in the documentation will be the two volumes here under review. A quarter-century ago British industries were beckoning to economists, but to British economists industries were out of bounds. In Manchester there were stirrings of systematic interest, but elsewhere, except sporadically, British economists were not greatly aroused. Today, this is changed and changing. No British industry needs to feel neglected; indeed, none is safe from analysis by British economic analysts who know how to analyze. Few of them as yet are confident of their prescriptions, but the analysis is vigorous and its quality is high. In this field, at this stage, acknowledged lack of confidence is one index of competence.

The present symposium of industry studies—nineteen industries under the micro- (and macro-) scopes of twenty-one economists—is one of the best. Sponsored by the National Institute of Economic and Social Research, and under the skilful editorship of Duncan Burn, the two solid volumes succeed notably in giving "an up-to-date picture of the structure of a group of

[representative] British industries—the number, size, scope, and interrelations of the firms or units within each industry, . . . and their relations with industries overseas, and with the government"; and in analyzing "the effect of the structure of each industry on its economic performance, its adaptability to markets and to technical change, and its contribution to such change." The industries covered are agriculture, building construction, inland road and rail transport, oil, chemicals, steel, building materials, machine tool, motor, aircraft, shipbuilding, electronics, cotton and rayon textile, woolen and worsted, man-made fibres, pottery, pharmaceuticals, and cutlery.

The studies are packed with pertinent information, but they are not statistical digests. Each is an independent unit, and the individuality of each writer is quite apparent in his chapter; yet each study makes its own complementary, even though quite distinctive, contribution to the whole work. Each chapter carries authority, yet each gains in authority from the others. The entire enterprise is characterized by functional, if not by formal, unity and by firm direction; and the reader is bound to suspect strong influence, effectively exercised, by the editor. The suspicion is further supported by the presence of a concluding chapter written by the editor (in addition to his two industry chapters on oil and steel) which "draws together and discusses what is said in the book on some problems of common interest: criteria of efficiency, . . . the forms and effectiveness of competition, the functioning of very large and of rather small firms, the impact of cartel-like activities, the differential effects of the structure of the market for new capital and of some forms of taxation, and finally the impact of the State as owner, supervisor, subsidizer, and buyer, with special reference to administrative problems and to the evolution of price and investment policies." This chapter entitled *Retrospect*, taken by itself must surely be rated as one of the most perceptive essays on industrial organization to appear in recent years. The total effect of the book is to give to the reader not only an understanding of the structure, operation and performance of present-day British industry, but to provide him as well with some deep insights into the whole fascinating, baffling problem of industrial organization in our modern economies.

It is not possible within the limits of this review to outline and comment on each of the nineteen studies and the concluding essay, although each invites and is well worth a complete review. It is equally impossible to refrain from a few of the observations suggested by the host of ideas which the symposium generates and forces the reader to entertain. One is constantly impressed by the rich and intriguing qualities of Great Britain as a living laboratory for the study of industrial organization. Britain has on display something of virtually everything, not only historically but contemporaneously. On what other shelves will we find in discrete packages such an array of structures—free enterprise, self-cartelized enterprise, government-influenced, government-regulated and government-owned enterprise, and all shades of each? And sad old enterprises, happy growing enterprises, and leaping new enterprises, all in manageable sizes, even the big ones? And in this setting, how easy it is to slip into attractive comparisons and spectacular contrasts between different in-

dustries, different times and different countries—and how dangerous!

It is certainly true that Britain offers us the best present contemporary laboratory for continuing studies of government ownership in a democratic society. Speaking of such studies, it is interesting to detect the typical change in approach as we move from analyses of "private" to analyses of "public" industries. Part of the difference is due to ingrained attitude, part to opportunity. Public industries appear as single cellophane-wrapped firms; private industries as tangled complexes of individual firms whose variegated wrappings tend to obscure as much as they disclose. Public industries belong to "us," not to "them"; every move is a matter of legitimate public concern; every decision is made not by the "system" but by an identifiable official responsible to "us." It is quite understandable that the standards of judgment, even those of economists, are more uncompromising when public performance is being appraised than when private performance is being traced through the wilderness of the market. We expect private industry to be "workable"; we expect public industry to measure up to our formulae and curves.

Reading Gilbert Walker on "Carriage by Road and Rail" reminds us again that of all the problem children of industrial organization transportation is by all odds the most intractable. To devise a sensible pattern of public regulation to superimpose upon the indeterminate mélange of competition, monopoly and legitimately vested interests that characterizes the hauling of goods and persons by air, road, rail and water simply defies doing and, worse, the attempt leaves us muttering to ourselves. We are still aware of no promising proposals either for relief of our frustrations or for processes by which relief is likely to be found. Next page, please!

There are other incorrigible problem children on the playground, too: how in the case of nationalized or regulated industries shall we establish measures of "enough" investment and "enough" output that are sufficiently precise to permit us to doze comfortably with our determinations. How soothing it is in the case of unregulated private industries to leave these troublesome matters to the decision of the competitive market—until the quiet is shattered by some such categorical proposition as that of Burns: "obviously there is no lack of competition among the motor car firms," and by the equally categorical assertion that the effectiveness of competition is to be tested by results rather than by industry structure. What do we really know about competition *as an economizing force*, and what do we really know about the motivating, measuring and guiding power of profit and loss as essential constituents of the competition concept? Can competition which, as a matter of policy is held "in check" by "competitors" or which is permitted by them to influence only quality and service, but not price, be equated with *economizing* competition?

If, as strongly suggested in this book, our theories of competition do not fit the facts, it will behoove us either to discover and name the economizing forces which have demonstrably taken the place of competition, or to devise forces to play the role which hitherto we have assigned to competition. It will not do for us to assume, on the basis alone of "performance" in an inflationary, expanding, war and cold-war-stimulated economy that everything will be all right

if we don't look too closely. Economizing is society's business, and society will do well to discharge its responsibility to itself in this regard if free men are to continue to set the conditions and mold the shape of their freedom.

On a reminiscent note, it is pleasant in this book to encounter some old acquaintances—the restrictive price and production schemes of the 'thirties. Some of them are still walking the streets, sturdy and bold as brass, some are hiding out from the Monopolies Commission, and some are just sitting out their ineffectual lives on park benches. It is interesting to speculate on just how justified some of us were to be so disturbed over their earlier antics. Of course, some of the more assertive of the clan, such as the arrangements in coal and steel, have in the meantime become canonized; and this leads one to wonder about the real significance of the tug-of-war over the political status of the British steel industry. Honestly, does it make much difference whether British steel is owned privately and regulated by the government with the advice and consent of the Steel Federation, or is owned by the government and run by its former managers with the advice and consent of the Steel Federation?

All of the above is to say to economists: read *The Structure of British Industry*. Read it and weep over the state of our science at its very best. Read it and rejoice over your choice of a profession. Nothing is more fun than a barrel of economists—if the country can stand it!

BEN LEWIS

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Pricing in Big Business: A Case Approach. By A. D. H. KAPLAN, JOEL B. DIRLAM, and ROBERT F. LANZILLOTTI. Washington: The Brookings Institution, 1958. Pp. xiv, 344. $5.00; paper, $2.00.

This volume is a companion to the 1954 Brookings study *Big Enterprise in a Competitive System*. The earlier study focused on the quantitative aspects of industrial structure, with special reference to the position of big business; this one is a detailed examination of big-business behavior, especially with regard to pricing its output.

The big-business candidates selected for study are 20 well-known corporations. Eight have assets exceeding $1 billion; nearly all are among the top hundred. All have been highly profitable business ventures. Over the period 1947-55 the group averaged a 22 per cent rate of return (before federal income taxes) on invested capital, and in no year did one of them report a net loss. But while all 20 were large and profitable, their most striking similarity was the willingness of all to cooperate with the Brookings staff. The largest in the group (General Motors) is 40 times as large as the smallest (Carrier Corporation). One (again General Motors) earned an impressive 51 per cent before federal income taxes; another (Swift & Company) earned a modest 12 per cent. Some (United States Steel, du Pont, Union Carbide and American Can) sell almost exclusively to other business firms; others (A&P and Sears, Roebuck & Company) sell directly to consumers. Among the 20 corporations the lines of products range from heavy earth-moving equipment to canned

tuna; within a single corporation (General Electric), from large public-utility generators to lamp bulbs.

Through interviews, questionnaires, memoranda supplied by company officials, and information available in public sources, the authors attempted to classify big-business pricing policies according to over-all company objectives. The attempt encountered numerous difficulties: company objectives changed over time; they differed between new and established product lines and multiproduct and single-product firms; and objectives articulated by company officials were not always consistent with most of the facts of company policy. Nevertheless, the authors found sufficient consistency to classify 13 of the 20 according to three overriding policy objectives: to achieve (1) a target return on investment, (2) stable prices and margins, and (3) a desired market position. The remaining 7 either priced to meet competition or relied heavily on nonprice competition.

While the classification of individual firms was often arbitrary—a fact readily conceded by the authors—the study's principal deficiency appears to stem from the approach itself. The predictive quality of microeconomic theory derives from the traditional assumption of profits maximization. It is perfectly clear of course that Standard Oil, International Harvester, du Pont, and other members of big business were subject to fewer constraints (public and private) in the 1890's than they are at mid-twentieth century. But while it follows that big firms must now seek profits by means which differ from those they once employed, it does not follow that firms generally no longer seek maximum profits. There is then no strong presumption that profits-maximizing is "operationally a less useful" (p. 129) working hypothesis than the set of objectives the authors substitute for it. For example, take the 7 firms whose pricing policies were assigned to the "target return" classification:

	Target Return	*Actual Rate of Return, 1947-55*
General Motors	15%-20%	24.0%
International Harvester	10%	8.7%
Aluminum Corporation	20%	9.7%
du Pont	"a reasonable return"	17.6%
Standard Oil Company, New Jersey	"a fair return"	12.1%
Johns-Manville	"at least past level of earnings"	14.4%
Union Carbide	no definite rate	11.7%

Four of the targets are much too evasive to hit, or indeed for any one to know when they have been hit. Two are higher than their respective firms customarily attain, so the firms presumably are pricing to earn as much profit as they can. The remaining firm has consistently earned a rate above its announced target, which suggests that the target is not an effective constraint. Moreover, all 7 firms produce a wide range of products, which tend to earn rates of return that vary inversely with the intensity of the competition with which they are confronted. The evidence that profits-maximizing is not operational—or not operating—is not convincing. The authors would have made better use of

the wealth of materials available to them had they accepted the traditional assumption as a tentative hypothesis, and then proceeded to test its validity in the light of the constraints on big business they subsequently encountered.

This aside, the book is highly informative. The material, much of which is new and unavailable elsewhere, is organized to illuminate with considerable clarity the way big business prices, and this was the authors' principal objective. While the authors do not explicitly set forth the public-policy implications of their findings, they nevertheless reach a policy conclusion: The large corporation, far from residing in that safe and comfortable haven of stable prices, steady sales, and assured rates of return, must contend with the dynamism of an ever-changing economy by which it is forced periodically to re-examine and change its policies. The findings indicate in substance: if one can safely generalize that market forces typically do not control big business, then an equally safe generalization is that big business typically does not for long control the market.

The volume marks a decade of diligent search for the facts on big business by A. D. H. Kaplan, its senior author. It also marks his retirement from the Brookings Institution's staff. It is to be hoped that it does not ring down the curtain on Brookings', or Kaplan's, published inquiries in this field. The searchlight of economists, and others, has been turned unceasingly on big business since well before the founding of the American Economic Association, but it has not yet revealed all the useful facts.

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Our Competitive System and Public Policy. By THOMAS J. ANDERSON, JR. Cincinnati: South-Western Publishing Co., 1958. Pp. vi, 586. $6.75.

The continuing quest for elusive causes of inflation has now moved into the field of monopoly and competition: trust busting, as it were, is fashionable again. Accordingly, it is a pleasure to report that Dr. Anderson has written an excellent volume that should be on the "must" list of any crusading Congressman.

This is not to imply that the book is preoccupied with questions of administered prices, market-sharing and the like. On the contrary, it presents a well-balanced treatment of all the traditional areas generally covered in a course on the social control of business. But what does differentiate this product is the author's successful attempt to integrate the body of antitrust material into the general framework of economic theory.

Typically, the course on monopoly and competition is a problem child in curriculum planning. At one extreme the course is structured along the lines of a legal study—with undue emphasis on the dates and circumstances of Supreme Court decisions. Theory is all but abandoned as case after case is cited in tracing the evolution of one or more of the various statutes. Opposed to this is the more recent attempt to set up the course as a study of market models, with legal precedents relegated to a few, scanty footnotes.

Anderson has avoided both of these pitfalls. If anything his bias is toward the institutional: history and the evolution of policy are indeed given their due. But these elements are carefully combined with basic economic principles, so that the result can be readily integrated into the main body of a department's offerings. An innovation for this type of book is the author's inclusion of several chapters on the competitive and monopolistic aspects of factor markets. In particular, the discussion of organized and unorganized labor markets is a model of clarity; the same may be said for the sober appraisal of the special problems of small-scale business. In analyzing these and similar matters he has chosen to use a descriptive approach entirely: there is not a single instance of the use of curves or diagrams in the entire book.

If any one section should be singled out for specific mention it is Anderson's critical evaluation of policy considerations and proposals made by various investigatory bodies since 1938. Alluding to the work of the T.N.E.C., the Twentieth Century Fund (Stocking and Watkins studies), and the Attorney General's National Committee to Study the Antitrust Laws ("Committee of Sixty"), the author points out that similarity of views rather than conflict characterized these three separate reports. Although Anderson does not offer any extensive set of suggestions for amending the antitrust laws, he does set up a balance sheet of sorts, outlining the major problem areas that continue to confront us.

It is this latter section that would well serve the needs of a fact-seeking legislator. For professional economists, the volume would seem to provide an excellent text or a fine reference for brushing up on this vital area.

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Readings in Industrial Organization and Public Policy. Edited by Richard B. Heflebower and George W. Stocking. Homewood, Ill.: Richard D. Irwin, 1958. Pp. xi, 426. $6.00.

This volume, selected by a committee of the American Economic Association, has two avowed purposes: (1) to provide the nonspecialist with a compilation of the more important articles in industrial organization and public policy which have appeared since 1942, and (2) to offer a book of readings for graduate courses in the field.

The articles selected are grouped into five partly overlapping categories: the structure of industries and markets; case studies in industrial structure and behavior; business practices and market behavior; industrial organization and economic theory; and competition, monopoly, and public policy.

In selecting the articles, the editors decided not to include sections of books, proceedings of conferences, and governmental hearings, although they did reprint some articles which had appeared in other books of readings. They placed a high priority on articles "based on important research and on articles developing significant viewpoints on analytical or policy issues," but a low

priority on "articles representing a digest or survey of the literature." In some instances, the editors included papers presented at professional meetings, together with the "criticism by formal discussants"; in others, such criticism was omitted.

In comparing this volume with its predecessor, *Readings in the Social Control of Industry* (1942), the editors underscore the changing conception of industrial organization and public policy. The older volume, they say, "was concerned almost exclusively with policy issues. Several of the articles dealt with the governmental problem of regulating prices, a topic omitted almost entirely from the present volume," where the emphasis is on "the characteristics of markets of few sellers and the policy problems growing out of them. Policy problems [in the current volume] are conceived as problems of antitrust rather than problems of government regulation. But as antitrust problems they are concerned with an industry's organization and economic performance rather than with traditional legal concepts of antitrust administration."

A book of readings is not likely to please everybody, and this volume is no exception. Different specialists, depending on their orientation and point of view, will inevitably have different ideas on the articles to be included and the topics to be emphasized. In fairness to the editors, therefore, a reviewer should do little more than comment on the quality of the selections (which in this volume is generally high) and on the standards used in making the selections.

My major criticism is that the articles in this volume lack unity and coherence. The editors are aware of this shortcoming, but attribute it to the imprecise boundaries of the industrial organization field and to the great variation in the content of graduate courses. This explanation is not entirely persuasive. The editors might well have chosen a few major *Leitmotifs*—e.g. the measurement of industrial concentration, the relation between size and efficiency, the economic criteria for antitrust policy—and presented divergent viewpoints on each. This would not only have made for greater organizational unity, but would have also enhanced reader interest. It would have impressed on the student the essentially controversial nature of this field and the sharp differences of opinion expressed in the literature.

Some major policy issues which are the subject of current and recurrent debate simply do not come to life in this volume. Though Congress, the courts, and the antitrust agencies are in constant turmoil over price discrimination and Robinson-Patman economics; though the economic and legal literature abounds with controversy on the subject, there is hardly mention of it in this volume. Neither the landmark effort of M. A. Adelman nor that of his critics, J. B. Dirlam and A. E. Kahn, are given any space by the editors. Administered prices, countervailing power, the role of competition in the regulated industries are treated, if at all, only in passing. The issues are never brought out in bold relief.

Finally, I must disagree with the editors' decision to exclude (automatically) materials contained in governmental hearings. In recent years, such hearings have become a favorite forum for the industrial organization

specialist. They contain not only important policy pronouncements, but also significant research findings of leading economists. The roster of witnesses, especially before the Celler, Kefauver, and Joint Economic Committees, reads like a "Who's Who" of the American Economic Association: George Stocking, Abba Lerner, Richard Ruggles, Fritz Machlup, George Stigler, Ben Lewis, Edwin Nourse, Kenneth Galbraith, Gardiner Means, and many others. Some of their statements are contributions fully as significant as the articles reprinted in this volume. The source in which they appear should not have been determinative of their selection or rejection.

Heflebower and Stocking have undertaken a difficult and unrewarding task. They have turned out a useful volume. The fact that some specialists would have used a different organizational pattern or a broader selection base should in no way detract from this effort.

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Land Economics; Agricultural Economics; Economic Geography; Housing

The Dynamics of Supply: Estimation of Farmers' Response to Price. By MARC NERLOVE. Baltimore: Johns Hopkins Press, 1958. Pp. ix, 267. $5.00; paper, $4.00.

Nerlove's book, *The Dynamics of Supply*, is on a subject most economists abstract from, or at best, talk about with the aid of textbook illustrative numbers. The neglect of dynamics by the profession can be largely explained by the difficulty of measuring "adjustment paths," rather than by the belief the subject is not significant. An approach, alternative to Nerlove's in economic dynamics research is the study of "special situations" in which nondynamic confounding disturbances can be disregarded as quantitatively insignificant.[1] Nerlove's approach emphasizes the use and development of analytical tools capable of sorting out dynamic and static components in normally available data.

This book is of value to persons concerned with economic dynamics defined, not as historical change, but with reference to the adjustment paths between equilibrium points following a disturbance when all other changes in wants, resources and technology are held constant. His analytical techniques need not be restricted to problems of supply—they can be applied to problems of income and demand as well. Specifically, this book deals with the (1) analytical problems of elasticity measurement, (2) supply elasticity measurements for corn, cotton and wheat, and (3) use of such data applied to forecasting, stability conditions and the evaluation of alternative agricultural price support programs.

[1] Examples of the "special situation" emphasis are: (1) G. K. Brinegar, "Income, Savings Balances and Net Savings," *Rev. Econ. Stat.*, Feb. 1953, *35*, 71-74; (2) C. H. Berry, et al., "Short Run Effects Following Controlled Price Changes: Skim Milk," *Jour. Farm Econ.*, Nov. 1958, *40*, 892-902. An example of the second emphasis is: Milton Friedman, *A Theory of the Consumption Function*, Nat. Bur. Econ. Research, Princeton 1957.

The author's major concern is with the problem of estimation, but as the author states ". . . the *raison d'être* of such estimation is application." His methodological contribution is centered on the use of distributed lags—relevant to much of economic research. His substantive findings suggest higher long-run supply elasticities than have usually been found. His application to policy illustrates, unintentionally I think, what not to do, as well as what to do. Chapters 1 and 2 concern theoretical considerations; Chapter 3, the application of this theory to agricultural commodities; Chapter 4 directs attention to the data and their limitations; Chapters 5, 6 and 7 present detailed background data on corn, cotton and wheat production and the accompanying government programs; Chapters 8 and 9 present the statistical analyses, with applications included as two-fifths of Chapter 8.

In the balance of this review some of Nerlove's other publications on this subject, along with the comments of others, are footnoted;[2] secondly, an observation on research methodology is noted; and lastly, one of Nerlove's applications to agricultural policy is criticized.

The distributed lags, both the technological-frictional ones and the expectational ones, can be usefully thought of as sophisticated, statistical grab bags in that they include a number of unknown elements. The content of these grab bags is similar to the content of statements that assert that something behaves *as if* such and such were the case, rather than similar to statements based on verified propositions. Thus the usefulness of the elasticity estimates will be determined by how well they stand up on the basis of continued testing—they can not be taken as useful because the underlying propositions have been verified. Lest this comment be interpreted to imply that an *as if* analytical procedure is unproductive, let me hasten to add this method has been used in the physical sciences with great success and with a significant measure of success in other areas. The pitfalls to be avoided are those of (1) shifting back and forth between the two methods inappropriately, thus neglecting to prove (more precisely, not disprove) anything, and (2) failure to know when these alternative techniques were employed.

The usefulness of the elasticity magnitudes obtained in this study must be evaluated in light of the above considerations. In a small section of Chapter 8, pages 222-35, Nerlove's study is extended to cover "Welfare Losses under Alternative Price Support Programs" by use of an illustration. Attention is directed to the part of his illustration that is a misapplication of his findings

[2] (1) Marc Nerlove, "Estimates of the Elasticities of Supply of Selected Agricultural Commodities," *Jour. Farm Econ.*, May 1956, *38*, 496-509; also comment by G. A. King, 509-11. (2) G. F. Brandow, "A Note on the Nerlove Estimate of Supply Elasticity," *Jour. Farm Econ.*, Aug. 1958, *40*, 719-22 and Nerlove, "Reply," 723-28. (3) Marc Nerlove, "Distributed Lags and Estimation of Long-Run Supply and Demand Elasticities: Theoretical Considerations," *Jour. Farm Econ.*, May 1958, *40*, 301-10 and "Comments," 311-14. (4) Marc Nerlove, "Distributed Lags and Demand Analysis for Agricultural and Other Commodities," Dept. Agric. *Agricultural Handbook No. 141*, Washington 1958; also review by A. S. Goldberger, *Am. Econ. Rev.*, Dec. 1958, *48*, 1,011-13. (5) Marc Nerlove and William Addison, "Statistical Estimation of Long-Run Elasticities of Supply and Demand," *Jour. Farm Econ.*, Nov. 1958. *40*. 861-80.

to agricultural policy. He assumes that the alternative uses of resources can be determined from a knowledge of supply elasticities—an assumption with which I have no quarrel when the elasticities are derived from and applied to situations in which "relatively free" markets exist. The author recognized this limitation when he calculated his elasticity estimates on the basis of pre-1933 data. He did not recognize this limitation in application when he presented some illustrative estimates of welfare losses when output is restricted through the use of quotas or direct controls. Supply elasticity estimates are used along with other data to compare welfare losses under direct control programs, direct payments programs and crop destruction programs. The trouble with this procedure, in making welfare comparisons, is that "free market" elasticities are not relevant to situations where direct controls are used. Resources denied use in the production of usual outputs, because of direct controls, may well have productivities near zero in their next best alternative uses. The productivity of these resources in their next best use will almost certainly be lower than that implied by accurate "free market" supply elasticity estimates. Useful data on the productivity of inputs released from their usual uses, as a result of quota impositions, can be obtained from other sources—one of the most direct being the prices of quotas—bootleg, as well as legal.

This volume, along with other literature on the subject, is helping to put new life in a much neglected subject that requires a lot of hard work. More publications of this sort are needed and should be encouraged.

George K. Brinegar

University of Connecticut

Perspectives on Conservation: Essays on America's Natural Resources. Edited by Henry Jarrett. Baltimore: The Johns Hopkins Press, for Resources for the Future, 1958. Pp. xii, 260. $5.00.

These essays by 23 authors are the effectively edited and revised papers and discussions of a forum organized by Resources for the Future and held in Washington, D.C., during the first three months of 1958. The forum consisted of six programs, each with a principal paper and two or three shorter discussion papers.

The six major papers are concerned with a historical retrospect of the conservation movement (Ernest S. Griffith), a survey of the role of technology in natural resources (Thomas B. Nolan), resource demand and living standards (John Kenneth Galbraith), urban growth and natural resources (Luther Gulick), the political economy of resources use (Edward S. Mason), and the administrative organization of resource development (Gilbert F. White).

Review of each individual paper and discussion is excluded by the constraint of space. In any event, such detailed reviews do not appear necessary because most contributors to this forum have expressed similar views previously elsewhere.

In appraising the volume as a whole, the editor, Henry Jarrett, states in

the introduction that the selection of the authors and the grouping of the subject matter "made it possible to look at old problems from new angles and with fresh eyes and to relate resource conservation more closely with the country's total economy and social structure and with world developments." This and similar statements disclose an unawareness of the pertinent literature—not uncommon in the staff writings of Resources for the Future. The particular selection of contributions is interesting individually; but it does not fulfill the claims and is scarcely a representative sample of the original and systematic work that is concerned with resources in the social and natural sciences.

The book under review contains perspectives on conservation not because "new angles" and "fresh eyes" are applied in observation, but because a multiplicity of often contradictory views is presented through the six programs, and within each program, through several discussions of the major paper. Perspective in this sense is the strongest feature of the volume.

As a corollary, symposia of this kind frequently suffer from a lack of systematic thinking through a subject matter. This characteristic is also the main weakness of these essays. One may submit that such thinking through is the greatest need in the social science aspects of conservation policy.

S. V. Ciriacy-Wantrup

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Agricultural Adjustment Problems in a Growing Economy. Edited by Earl O. Heady, Howard G. Diesslin, Harald R. Jensen, Glenn L. Johnson. Ames: Iowa State College Press, 1958. Pp. xv, 315. $3.95.

This book is an assembly of papers presented at a conference on "Adjusting Commercial Agriculture to Economic Growth" and published under the sponsorship of the North Central Farm Management Research Committee. This conference was a follow-up to an earlier one on "Resource Productivity, Returns to Scale and Farm Size," the report of which was published by the Iowa State College Press in 1956.

The first paper by Earl O. Heady and Joseph Ackerman deals with the income and resources-allocation problem. Its most significant contribution is a clear statement of the position of the farmer in relation to national economic growth and the need for the transfer of resources from the farm to other segments of the economy. Farmers have made significant contributions to an expanding national economy without sharing equally in the fruits of growth. In fact the demands for some farm products have declined while the costs of goods and services employed by farmers in production have generally increased. This has resulted in a cost-price squeeze on the farmer. The conclusion is that some resources must be transferred out of agriculture.

Recent developments in agricultural production are reviewed by Lynn S. Robertson and Howard G. Diesslin. The trends in reorganization have been in the right direction but too slow. The effects of the transfer of resources have

been more than offset by the effects of technological research and education upon the productivity of the resources remaining in agriculture. The paper by Sherman Johnson and Glen Barton dealing with the effects of research and education presents an excellent analysis with suggestions for directing more research toward resolving or reconciling the conflict between national progress and income improvement for the farmer.

The prospective demand for farm products in relation to prospective supplies is examined by Norman Collins and George Mehren to determine the extent to which the growth of population and other sources of potential increase in demand might contribute to the solution of the problem. After careful analysis it is concluded that total consumption might increase by 40 to 45 per cent in the next 20 years; but production, now in excess of consumption requirements, is likely to increase even more. The conclusion is that there is no effective method of solving the problem through increasing demand. Therefore, it is necessary to seek a solution through adjustment of farm production.

The analysis of the supply function in farm production by Glenn Johnson is perhaps the most significant breaking of new ground presented at the conference. The new ground is in extending the theory and analysis of fixed assets in relation to production and the position of the farmer.

Willard Cochrane presents an interesting paper on some additional views on supply and demand including significant comments upon the papers by Glenn Johnson and others dealing with demand in relation to supply.

The internal structure of agriculture is analyzed to determine what reorganization of the use of resources might contribute to the solution of the problem. One suggested solution is to shift surplus cash crop resources to livestock production. James Bonnen indicates that only about one-quarter of the annual crop surplus could be absorbed in such a shift without serious consequences to that industry. He also concludes that taking land out of cultivation is not the answer because other resources are readily substituted for land to maintain production by increasing yields. Capital also is readily substituted for labor in maintaining or increasing production. The conclusion of this analysis is (p. 126): "Any effective effort to reduce production must involve the simultaneous transfer of some combination of labor, land, and capital resources to nonagricultural pursuits."

Reduction in the labor force as a means of solving the farm problem is discussed at length. Some agricultural economists consider that shifting labor to other cccupations is the best solution to the farm problem. In the first paper (by Earl Heady) dealing with this proposal it is noted that in the last 20 years a reduction of about one-third in labor input has been accompanied by an increase of 38 per cent in output. That is, technological improvements and the substitution of capital for labor have more than offset the migration of labor to industry. It seems clear, therefore, that the rate of migration must be increased substantially to be effective in reducing output.

Among the ways and means of increasing labor mobility discussed by D. Gale Johnson are: improvement in the educational facilities of low-income

areas, extension of information as to job opportunities, and loans or grants in aid of transfers. An analysis by Vernon Ruttan concludes that rural industrialization and local economic development may make significant contributions in some areas but longer distances or geographic mobility are necessary to relieve agriculture generally of surplus labor.

An analysis of current adjustment programs by G. E. Brandow indicates that they are not contributing much toward solving the surplus production problem. The soil bank has contributed to some extent to reducing the acreage of some crops, but price maintenance has encouraged the maintenance of production through increasing yields per acre. It is suggested that the programs be revised so as to be more effective in encouraging adjustments of supply in relation to requirements at prices that will improve the income position of farmers.

There were also papers presented which dealt with individual and social goals in relation to economic growth, the farmers' social and political psychology and the value problem in agriculture policy. One paper, by C. B. Baker, deals with the relation of the individual's interest in income to the social interest in the national product. A second, by W. R. Parks, emphasizes the interest of the farmer in security and deals with the problems of the farmer in obtaining political recognition of his interests. Equality of opportunity is emphasized by Kenneth Parsons; and he believes that the means of obtaining adjustments should be considered as important as the end results.

In summary the implied long-run objective of this conference was to suggest ways and means of eliminating surplus production so that farmers may obtain more adequate income in the free market. The objective for agriculture is to reduce production to levels in line with current requirements and reduce the unit costs of the products. Fewer farmers with larger farms and more extensive use of capital could supply the national needs of a growing economy from agriculture and receive more adequate net incomes in the free market.

Desirable adjustments are under way but they are too slow. They should be speeded up through research, education and extension services more specifically directed to adjustment problems. Government financial aid to the migration of surplus farm labor is also suggested. The present government programs should be revised to be more effective in encouraging and aiding adjustments.

The most significant contribution of this book is in bringing together analyses of various aspects of the agricultural adjustment problem. The several participants in the conference have written and spoken extensively in their special-interest fields. In this conference an attempt was made to concentrate the attention of each analyst on the relation of his special interest to the overall problem of adjustment. The result is a good reference book for economists and other students of agricultural economic policy.

O. C. Stine

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Introduction to Agricultural Economic Analysis. By C. E. Bishop and W. D. Toussaint. New York: John Wiley & Sons, 1958. Pp. xiv, 258. $5.25.

The authors' objective is "to provide a theoretical foundation for use in analysis of agricultural economics problems. . . . Economic theory is presented in an elementary but rigorous form, and illustrations are developed to show the use of theoretical concepts in solving empirical problems." Written primarily for sophomores and juniors, without prior exposure to formal economic analysis or mathematics, the book includes four sections: (1) economic organizations, the nature of decision making, and agriculture as a part of the general economy; (2) production and supply, utilization of productive services, cost functions, and a bit on uncertainty; (3) consumption and demand, international trade, and long-term and short-term price movements; (4) technological influences, population, and income with attention to the low-income areas in agriculture.

Sections 1, 3, and 4 are treated in broad terms and rather lightly. The heart of the book, and its main contribution as an instruction vehicle, is in section 2 which constitutes about 60 per cent of the entire volume. Following the long-established tradition in agricultural economics, it presents a more detailed discussion of what is commonly referred to as production economics than is found in most introductory texts on economic principles. Although oriented to concepts and analysis, the section on production and supply includes a very generous portion of examples selected from empirical studies by various researchers in the economics of agricultural production and resource use.

Another feature of the book is its more than usual attention to multiple products and uncertainty in production decisions, compared with most introductory texts on economic analysis. Although the beginning student is given reasonably detailed exposure to short-run production analysis, the treatment of long-run analysis will have to be supplemented heavily by the instructor's lectures.

In contrast with the rather detailed treatment of production and supply (at an elementary level, but certainly not rigorous) the explanatory exposition of consumption and demand is slim and sketchy. The student is made aware of price elasticity and income elasticity. Long-term changes in per-capita consumption are attributed primarily to changes in consumers' preferences and changes in income. The role of relative prices in influencing changing consumption patterns is neglected. Cross elasticities of demand with respect to price, for example, are not recognized in spite of the evidence that consumers do shift among food products in response to changes in relative prices as well as in response to changes in income. The discussion of demand will require shoring up and fortification by the instructor.

In terms of market structure, the analytical parts of the book deal almost exclusively with pure competition in buying and selling. The student is not exposed to the analysis (and problems therein) of alternative forms of market structures. Yet, a case can be made that American agriculture is breaking away from pure competition, with developments stemming from the growth of pro-

curement and marketing cooperatives and the extension of forward and backward vertical integration in addition to governmental participation in various forms. Some interesting and significant examples of and problems in nonpure competition may be drawn from the contemporary scene in American agriculture. But how economists struggle with the analysis of such problems is omitted from the book; the authors leave that task to the instructor.

For an elementary text heavily oriented towards production and supply for a firm in a purely competitive environment, the book has much in its favor, particularly the wealth of illustrations to show the use of theoretical concepts in analyzing empirical problems. To give the student a balanced introduction to agricultural economic analysis, however, the instructor will likely want to plug some significant gaps by his lectures and with the use of supplementary study materials.

SIDNEY HOOS

University of California

Lesnaia promyshlennost' SSSR: Statisticheskii sbornik. (The Timber Industry of the USSR: Statistical Handbook.) Moscow-Leningrad: Goslesbumizdat, 1957. Pp. 295.

Since 1956 the Soviet Union has released considerable economic information in the form of statistical handbooks. The timber handbook is one of the few dealing with a specific industry. For the most part, only favorable information is reported. Extensive tables describe the large quantity of forest wealth of the USSR, and the shift of logging activity from the "timber-deficit" areas of the South and West to the "timber-surplus" regions of the North and East. Impressive data indicate the manifold increase in total logging and wood-products output as well as the growth of logging mechanization within the timber ministry. (Since publication of the handbook the ministry has been abolished in line with Khrushchev's decentralization of administration.) Figures on the logging labor force indicate the virtual elimination of seasonal workers.

The favorable information presented is, in fact, riddled with statistical uncertainties. While outright falsification is apparently absent, the figures are frequently used in ways outside the bounds of Western statistical practice. For example, important qualitative characteristics of Soviet forests are obscured. Although total forest area and volume are divided into age classes, the important fact that much of the mature timber is small is not indicated. Also ignored are the large quantities of waste-wood in the vast stretches of overmature and swampy taiga. Figures on logging mechanization fail to indicate that only certain processes have been mechanized and that other processes of increasing importance remain completely performed by hand labor. In Soviet terminology, the "mechanization of production" has increased while the "mechanization of labor" remains low. While figures show the growth of a permanent labor force in logging, collateral information from Soviet timber

industry periodicals indicates a continuing high rate of labor turnover, much of which is interindustry turnover.

Considerable information of importance is not disclosed. Production costs are treated only sketchily. For example, a table is presented showing costs in the entire "factory-plant" branch of the timber ministry. Presumably, products included range from wood chemicals to yo-yos. Pricing of wood products and the policy on stumpage charges are not examined. Such information is absolutely necessary, however, if we are to assess the timber industry as a functioning branch of the Soviet economy because the industry is tied to the economy by just such centrally determined prices. No data are provided on regional or even total forest growth. Understandably, no mention is made of the role of "forced labor" in logging, although it must be considered in evaluating figures provided on labor productivity. A pervasive and serious limitation of the handbook is that only centralized wood enterprises are considered (except for figures on total production). In 1957 almost half the logging output was carried out by "self-suppliers," i.e., enterprises in steel, food, fishing, etc., outside the ministerial apparatus, which require wood in the normal course of production but have been unable to obtain it from central sources of supply. Nor is any information other than total production figures provided on agricultural timber supply, certainly a fact of great importance given the natural and institutional vicissitudes of Soviet agriculture.

Ten thousand copies of the handbook were printed, leaving few for circulation abroad after domestic requirements had been satisfied. Undoubtedly, the handbook is intended primarily to serve the needs of its domestic readers; thus to object to the lack of definitions of statistical categories utilized is to quibble. No one would accuse the Department of Commerce of obscurantism, e.g., for failing to define gross national product or disposable income each time those terms were used. Similarly, the frequent conversion of absolute figures into relatives may annoy Western readers. Perhaps we should not discount the possibility that many Soviet users of the handbook may lack mechanical means to perform just such simple computations. The absence of figures on self-suppliers and other items of interest possibly reflects weaknesses in the data-gathering process.

Finally, it must be remembered that the text was published to serve Soviet interests, but to do so in such a way as to protect the regime from criticism. The handbook must be used with great selectivity and restraint if meaningful conclusions are to be drawn from the data presented.

W. Donald Bowles

The American University

Labor Economics

As Unions Mature: An Analysis of the Evolution of American Unionism. By Richard A. Lester. Princeton: Princeton University Press, 1958. Pp. xi, 171. $3.75.

The time is ripe for a study of labor's "coming of age." In the 'thirties the prolonged infancy of unionism finally gave way to stormy adolescence. Only

in the last two decades can it be said that unions have achieved power, status and maturity, but with such maturation has come widespread public demand for "responsible" union behavior.

Lester's analysis of union maturity is not, however, designed to forge a standard against which union performance can be judged. Rather, his purpose is to evolve a theory of union growth. At the outset he notes the deficiencies of existing theories of unionism: Hoxie failed to reconcile competing forces operating on the union movement; Perlman's theory is said to be unnecessarily static with its focus on job consciousness; and Tannenbaum's analysis is largely historical. In his own "evolutionary" theory he indicates that the character and philosophy of unionism are influenced by the institutional setting in which the union movement finds itself. In his mind, a "theory" of unionism must be sufficiently pliable to explain labor's adaptation to continuing changes in the labor market.

Throughout his study Lester stresses the political aspect of unionism: Union success is measured not so much by economic power as in the capacity of the union to win certification elections, to be politically sensitive to employee needs, and to be politically effective in its relationships with the public and Congress. ". . . the theory of union behavior should ultimately extract more sustenance from political theory than it has been able to draw from economic models based on the theory of the firm" (p. 132).

The core of Lester's analysis is the development of a union growth curve. In his view, unions at the outset face a hostile and turbulent environment and overcome opposition through spirited militancy. A youthful energy and crusading spirit infuses the organization, giving it momentum and direction. Leadership displays a missionary zeal. Should the permanence of the union be assured, however, the aging process begins in two or three decades. The locus of union power shifts from the local to the national office; centralization discourages rank-and-file participation in union affairs and makes possible the building up of a political machine. The adjustments of the union member to his new union bureaucracy—usually made imperceptibly through time—has its counterpart in the adjustment of management to the union. Less and less resort is made to strike activity; the mutual interests of the union and management are made apparent (on occasion to the point where the union may not secure wage increases equivalent to those possible with the free play of market forces). The national office of the union insists on strict adherence to contract terms and in one sense becomes an element in management's control system. Ultimately the union disregards its class orientation in its accommodation to both management and society. The bargaining process shifts from table-pounding emotionalism to reasoned analyses of statistics, law and economic theory. The union staff specialist assumes additional importance: policy is made from the top and is sold, through skillful communications, to the membership. The union faces less of a challenge from management, less opportunity (and perhaps less need) for factionalism within, less rivalry from other unions. And the security of the union makes possible the security of the union officer.

The material success enjoyed by union officers is a further conservative force. Officers acquire the attributes of success and the prestige symbols of

management. And with labor's subsistence needs satisfied, union interests becomes diversified. As the union is no longer a microcosm of society but rather a substantial cross-section of it, it imbues and is imbued by the values of the community. It becomes concerned with the wider and more diffuse problems of all society, diluting its provincial "union-only" orientation.

The total effect of these forces is to reduce the pace of union growth. Barring radical change in our economy, Lester sees no increase in real union membership in the next ten years. As a test of his analytical framework, Lester provides a brief case study of unionism in Sweden and England and follows this with a thumb-nail sketch of the Amalgamated Clothing Workers, the United Automobile Workers, the Carpenters, the Teamsters and the Mine Workers. He finds that the organizational history of unions, both here and abroad, fits pretty well into his conceptual framework. Even the youthful Automobile Workers' union displays a few symptoms of this aging process.

Lester's analysis is not based on fresh research but is more, as he himself describes it, a "think piece," and is, in reality, a conversational review and reflection of his extended study of the labor movement. There is little in his analysis that is novel, for it largely represents a synthesis of existing research in union government. His task has been largely one of aerial reconnaissance or a mapping out of terrain. As such, *As Unions Mature* provides an appropriate point of departure for students planning further explorations of contemporary collective bargaining.

PAUL SULTAN

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National Wages Policy in War and Peace. By B. C. ROBERTS. London: Allen & Unwin Ltd.; New York: Macmillan, distrib. Pp. 180. $3.50.

This book is another contribution to the discussion of inflation, its causes and the alternative policies for its control, in which economists have frequently engaged since the end of the second world war. For the first time, however, a comparative study of what has been done and its degree of success in several different countries has been made. As the title of the book suggests, Roberts examines specifically the relationship between wages policy and the problem of maintaining economic stability during both wartime and peacetime.

The author studies the wage controls and wages policy in Britain during the war and under both Labour and Conservative governments, American wartime policy and the wage stabilization program of 1950-1952, experience in Sweden, Australia, Netherlands and West Germany. And with the exception of West Germany he finds that the change in the price level between 1939 and 1957 has been about the same in each country and irrespective of the wages policy. Australia has used a system of compulsory arbitration; Sweden a coordinated program of voluntary agreements between national trade union federations and national employer associations; Holland has adopted a comprehensive system of statutory wage regulations. Both British and American centrally administered wartime wage controls were unable to bottle up infla-

tionary pressures. Efforts in Britain by both the Labour and Conservative parties to effectuate a national wage policy of "wage restraint" by the unions or one of wage controls have been without success in checking the rise in prices. Accordingly the author concludes that, "Whatever in theory might be said in favour of a centralized system of wage controls, it cannot be said that those examined . . . have succeeded in preventing inflation" (p. 160).

If wages policy cannot check inflation, then what? Roberts argues that the root cause of price increases in the countries studied was not "wages rising faster than output, leading to an increase in labour costs that is passed on to the consumer in the form of higher prices" (p. 161). Rather, increase in the supply of money, brought about by the conscious political decision of the monetary authorities (p. 162) and "the transfer from idle to active balances and the increase in the turnover of deposits and cash in circulation has financed the continuance of inflation by building up the level of demand to meet each new [cost-induced] price level" (p. 163). In other words, "the problem [is] one of preventing the generation of excess demand . . ." (p. 164).

Roberts recognizes that an "attack on inflation by curtailing the supply of credit and capital expenditure will have the immediate effect of checking output." But he says that "this price may inevitably have to be paid to achieve stability" (p. 165). He sees no necessity, however, for average unemployment rates to rise much in the long run if swings about the average are allowed.

The choice which the author offers between inflation and a small rise in average unemployment is, however, oversimplified and incomplete. Roberts does not discuss what might happen to the distribution of income between labor and property, or between union and nonunion workers, and to the allocation of resources among various sectors of the economy. He implicitly assumes, for example, that unions will be unable or unwilling to alter the distribution of income. But this is not certain. The very expansion of the money supply since 1945 may well explain why unionism has been unable to influence the structure of wages substantially. Certainly a "get tough" monetary policy will not necessarily reduce or contain the unions' perpetual demand for higher real wages or a larger share of national product. Even though unions respond to unemployment, as the author indicates, this is a matter of degree. As union rates go up, new workers are denied jobs by employers; unemployed union members seek work elsewhere, and the pressure of "excess" supply in nonunion markets holds real wages below those pushed up by the unions. Only if the momentary authorities will accommodate the autonomous union wage increases by a corresponding expansion in the money supply will the growth in aggregate demand be sufficient to permit an increase in all factor prices in such a way as to avoid a significant change in the distribution of income.

In summary, there is a great deal to command in this book and very little to criticize. It is excellently written, well organized and easy to read, authoritative and well documented, and provides a comprehensive survey of the institutional arrangements, the wages policy and anti-inflation measures, as well as a wealth of factual data, in the six countries covered. Although

written primarily for a British audience and as an analysis and guide to policy in Britain, the book merits the close attention of American readers who are interested in the problem of inflation control and the role of unionism in achieving economic stability.

KENNETH M. MCCAFFREE

University of Washington

Les traitements des fonctionnaires et leur détermination (1930-1957). By ANDRÉ TIANO. Paris: Ed. M.-Th. Génin, Librairie de Médicis, 1957. Pp. 554. 3,500 fr.

Since its hard core was established by the first Napoleon, the French civil service has weathered revolutions and restorations, the rise and fall of countless governments, and a number of efforts to reorganize the service itself. Professor Tiano, one of the rising generation of French labor economists, has devoted this book to an exhaustive study of wage trends, wage structure, and wage determination in this hardy perennial institution.

Tiano estimates that *fonctionnaires,* together with their retired list, dependents, and recipients of survivors' benefits, made up more than 11 per cent of the French population in 1947, with their total receipts from the government comprising about the same percentage of net national income. In addition to civil servants in the American sense, it should be noted that *fonctionnaires* include the armed forces, telephone and telegraph, radio and television, and public schools from kindergarten to university.

The first half of the book is descriptive. The main findings are that real wages in public service were about the same (in terms of basic salary scale) in 1956 as in 1930, though with big swings in-between; that salary scales had roughly kept pace with wage rates in private employment over time, but were generally a bit lower for similar work in the past decade than in private or nationalized establishments; that wage differentials had been compressed; that pay varied greatly from agency to agency. Certain estimates are based on personal interviews as well as official data, and "extracurricular" sources of income are treated with Gallic frankness.

Real incomes have increased more than statistics of basic salary would indicate, but the chances for extra money vary from agency to agency and job to job. Overtime pay in lower grades and flat-rate overtime allowances in higher ones (whether overtime is worked or not), efficiency premiums, free housing for certain officials and residence allowances for others, shoe-wear allowances for postmen and extra pay for professors who grade more than their quota of bluebooks, when taken together with family allowances, mean that either a doorman or a director-general can sometimes double his basic salary. Economists will be glad to learn that the chance of doing so is greatest in the Economic Council; teachers can take pleasure in the fact that their French colleagues have been up-graded steadily through the years and can supplement their salaries by tutoring or by doubling as town clerks.

The second half of the book is devoted to wage determination. While

the percentage of union members is higher in public service than elsewhere, the weaker Socialist and Catholic unions are more strongly represented than in the country as a whole. Though joint administration-staff committees were set up in 1947, the unions prefer to press their claims through other channels. Strikes are usually short and symbolic, since the right to strike is limited, and rivalry between unions and between branches and grades within the service tends to weaken solidarity. The *fonctionnaires* also exert political pressure, sometimes through deputies with ministerial ambitions who—at least during the Fourth Republic—tended to push the claims of the ministry of their choice, sometimes through priests or civil servants sitting as deputies.

Wage determination is seen as a process of jockeying between administrations (interested in keeping within their budgets and still getting the work done) and unions, whose claims are backed by parliamentary pressure, appeals to public opinion, and strikes. Tiano concludes that higher officials lean heavily on civil service unions and unions grouping higher-level employees in the public and private sectors, that lower officials have more solidarity with the working class as a whole, and that there is some evidence that the post-telephone-telegraph unions exert leadership as regards strikes in both private and nationalized sectors of the economy.

Tiano's conclusions about wage determination may be open to question, but as a work of documentation the volume deserves serious consideration. American economists who can read French and have the patience to wade through the alphabet soup of French government agencies and the numbers and letters which identify salary scales, the intuition to make up for incomplete table headings, and a reading-glass for some extremely complicated and crowded graphs, will find themselves rewarded with a wealth of statistical and documentary information which is available in no other single source.

A. Peter Ruderman

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Population; Welfare Programs; Standards of Living

The Population of Japan. By Irene B. Taeuber. Princeton: Princeton University Press, 1958. Pp. xx, 461. $15.00.

The publication of Mrs. Taeuber's *magnum opus* has been eagerly awaited by economists, demographers, and various kinds of regional specialists. Preceded by a lengthy series of tantalizing and authoritative articles, *The Population of Japan* gives the author her first chance to present a full-dress, detailed and integrated picture of Japanese demography. She has made the most of this opportunity. This book lives up to the high standards which one has learned to expect from Princeton's Office of Population Research, and it is quite safe to say that Mrs. Taeuber has written the definitive work about Japanese population for at least a decade. (I shall be immensely surprised if a Japanese translation of this work does not appear in very short order.)

This study encompasses an impressive variety of topics. The author leads off with a fascinating discussion of population in the premodern period, followed by an analysis of the transition era from 1852 to 1918. Detailed work starts with the results of the first modern census of 1920, and here Mrs. Taeuber broadens her methods considerably. She discusses the changing composition of the population, underlining the economic aspects, which indeed form a sort of leitmotiv to the entire volume. There are long sections on internal and external migrations of the Japanese people, with the former providing a vivid picture of urbanization. Natural demographic movements—marriage, fertility, mortality, and natural increase—are all studied. A section also examines Japan's demographic ordeal during the war and in the postwar period, and discusses current policies and future problems. Note must also be made of an extremely valuable bibliography covering Japanese and foreign sources.

For obvious reasons the period since 1920 is heavily emphasized in this work. Mrs. Taeuber is quite pessimistic about whether precensus population data are accurate enough for meaningful analysis. Furthermore, her numerous, cautious, and well-documented conclusions rely primarily on census benchmarks; yearly series are infrequent. It would be foolish to take issue with her in these matters; but let us say that for quantitative economic historians certain large questions remain unanswered. For example, the process of urbanization before 1920 remains something of a questionmark.

Some of Mrs. Taeuber's most valuable insights come about through her regional classifications. She divides the prefectures of Japan into four principal categories—metropolitan, other industrial, intermediate, rural—and finds interesting demographic contrasts and similarities. In examining urbanization, the mechanical administrative *shi* and *gun* classifications are largely abandoned, and instead more rational typologies are employed in analysis. Here the economist must doff his cap in envious admiration, and conclude that population census-takers have dealt more kindly with demographers than economic statistics collectors with economists. In measuring economic development in terms of, say, national product or capital formation, the economist must usually rely on output figures which do not permit a breakdown in other than fairly large administrative units. This was certainly the case with the prewar census of manufactures in Japan. And it is of course also true that the prefectural statistics of Japan contain much more demographic than economic information. One must conclude that frequently it is not possible for quantitative economists to make direct use of demographic advances. However, there can be no question whatever about the over-all value of this type of analysis for the study of economic growth.

Mrs. Taeuber's findings and opinions are so rich and diverse that it is impossible to attempt an enumeration. But one further aspect of her work deserves comment. All through the book—implicitly and explicitly—she stresses duality, by which one means the intermingling of modern and traditional aspects in Japanese society. From the point of view of demography,

this may have meant that there are often no sharp divisions between urban and rural areas, or that among certain classes of workers in metropolitan areas rural characteristics of employment and reproduction have persisted for a long time. Similar characteristics are of course found in the economic analysis of Japanese development. The persistence of the demand for traditional commodities, ranging from houses to certain types of food, the apparent need and consequent indestructibility of the small (often midget) unit of enterprise, and perhaps the entire system of labor relations are different sides of the same coin. Certainly not all the aspects of this duality are adverse to industrialization; some may have made a vital contribution, and need to be considered in the general study of development policies.

I conclude with a minor complaint. Mrs. Taeuber's contribution could have been enhanced by the inclusion of a chapter comparing Japan's demographic experience with that of other industrialized countries. This reader's perspective would have benefited.

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The Older Population of the United States. By HENRY D. SHELDON. Census Monograph Series for the Social Science Research Council in cooperation with the Bureau of the Census. New York: John Wiley; London: Chapman & Hall, 1958. Pp. vii, 223. $6.00.

Like the other monographs in this series, the objectives set for this volume are two-fold: the presentation of detailed, statistical information collected by the Census and other public and private sources on a particular subject of current interest; and the "broad exploration of the new questions suggested by the new information, as well as narrowing the elements of doubt and controversy on old questions" (p. vi). The present volume fulfills both objectives. The pertinent issues are raised and the data analyzed against a broad background of knowledge and insight. The authors never lose themselves, and consequently they do not lose the reader, in a maze of detail.

The material is skillfully organized to show the position of older pepole in those aspects of the society on which the census gathers information—age structure, geographical distribution, employment, occupation, marital status and the family life cycle, living arrangements, housing, and income. In each setting the position of older people is analyzed within the context of the processes which have contributed to their present status and may influence future trends.

There are a few surprises. For example, self-employment appears to be less favorable for occupational longevity than is commonly supposed. Those who worry about the growing proportion of older people in the population will be relieved to learn that this is by no means certain. For the most part, however, the findings do not refute familiar generalizations. They limit and refine them. Analysis of the varied effects of migration on age structure by states and type and size of communities is particularly noteworthy, as is also

the use of survival rates to measure the degree to which older people continue in former occupations and the extent of employment opportunities in particular occupations. Relationships between age and labor-force participation still defy generalization, except for the obvious statement that declining energy and health are major factors in retirement, and even here the subtler relationships between health and employment remain nebulous. Situations confronted by older people with changes in family status are sensitively treated. The growing proportion of older people without earnings indicates the need for a broader system of support. The chapter on housing shows up the inadequacy of data on this subject.

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Contributions by: L. Couffignal, D. and A. Gabor, C. B. Gibbs and A. W. Phillips.

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Industrial Organization; Government and Business; Industry Studies

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Population; Welfare Programs; Standards of Living

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Related Disciplines

Marshall, H. D. Publication policies of the economic journals. Am. Econ. Rev., Mar. 1959, pp. 133-37.

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NOTES

A nominating committee consisting of John D. Black, chairman, Kenneth D. Roose, Alice E. Bourneuf, Raymond T. Bowman, James M. Buchanan and George Garvy has submitted the following slate of nominees for 1960 officers of the American Economic Association:

President:
Theodore W. Schultz

First Vice President:
Paul A. Samuelson

Vice President:
Lester V. Chandler
William J. Fellner
Raymond W. Goldsmith
Robert A. Gordon

Executive Committee:
Armen A. Alchian
John K. Galbraith
Frank C. Pierson
Ralph A. Young

The annual meeting of the Association will be held at the Sheraton-Park Hotel, Washington, D.C., December 28-30, 1959.

Norman S. Buchanan Memorial Award

The services of the late Norman S. Buchanan to teaching and research in economics and to the strengthening of the social sciences are being commemorated by his friends through the establishment of a Norman S. Buchanan Memorial Award, at the University of California, Berkeley, in the form of a prize or scholarship for excellence in economics. Contributions to this fund, which are tax exempt, may be sent to: Philip E. Mosely, Chairman, Norman S. Buchanan Memorial Committee, 58 East 68th St., New York 21, N.Y. Checks should be made out to The Regents of the University of California.

New Publications

A new journal, *Labor History,* sponsored by The Tamiment Institute in cooperation with the Society of Labor Historians, will be published in late 1959 or 1960. It will publish, in addition to original research in labor history, studies of specific unions and the impact of labor problems upon ethnic and minority groups; articles on the theory of labor history; and studies of foreign labor movements. Contributions may be addressed to the editorial director, Dr. Norman Jacobs, care of The Tamiment Institute, 7 East 15th St., New York 3, N.Y.

The Social Sciences—A Journal of Student Research, a periodical designed specifically to broaden the outlet for student research in the social sciences, will be published in the near future. Articles submitted should not exceed 2,000 words in length and should be typed in a form meeting publication standards. All communications and manuscripts should be sent to Professor Edward Rothstein, Department of Sociology, Dickinson College, Carlisle, Pa.

Announcements

The Fund for Social Analysis is offering in 1959 a number of grants-in-aid for studies of problems posed by Marxist theory and its application. Projects for books and essays in all fields of social science will be welcomed. Grants will ordinarily range from $500 to

$3,000, and may be requested for an entire project, or for any part, or for assistance in research, editing or publication. Address the Corresponding Secretary, The Fund for Social Analysis, Room 2800, 165 Broadway, New York 6, N.Y.

Section K—Social and Economic Sciences—of the American Association for the Advancement of Science will hold sessions for contributed papers at the annual meeting of the AAAS in Chicago, December 26-31, 1959. Association members interested in presenting a paper at these sessions should forward titles and abstracts, not later than September 20, to Donald P. Ray, Secretary, AAAS Section K, National Academy of Economics and Political Science, George Washington University, Washington 6. D.C.

Deaths

W. C. Beatty, November 13, 1958.

Sidney D. Merlin, December 8, 1958.

Ragnar Nurkse, Columbia University, May 6, 1959.

A. C. Pigou, King's College, Cambridge, March 7, 1959.

John G. Rolph, November 2, 1958.

Clyde O. Ruggles, emeritus, Harvard Graduate School of Business, April 6, 1958.

James MacD. Terrell, November 1958.

Leonard S. Tyson, February 1958.

Julius V. Wyler, New School for Social Research, January 13, 1959.

Retirements

Ruth A. Allen, University of Texas, June 1959.

Clyde O. Fisher, Wesleyan University, June 1959.

Reid S. Fulton, The City College of New York, June 1959.

H. L. McCracken, Louisiana State University, June 1959.

W. H. Steiner, Brooklyn College, June 1959.

Horace Taylor, Columbia University, June 1959.

Sam H. Thompson, Iowa State College, March 1959.

Kossuth M. Williamson, Wesleyan University, June 1959.

Promotions

John E. Bishop: assistant professor of business administration, Harvard Graduate School of Business Administration.

Donald E. Cullen: associate professor, New York State School of Industrial and Labor Relations, Cornell University.

Francis W. Gathof: assistant professor of economics, The American University.

Charles J. Grayson, Jr.: assistant professor of business administration, Harvard Graduate School of Business Administration.

Neil E. Harlan: associate professor of business administration, Harvard Graduate School of Business Administration.

Werner Z. Hirsch: professor of economics, Washington University.

James E. Howell: associate professor of economics, Graduate School of Business, Stanford University.

Duncan M. MacIntyre: professor, New York State School of Industrial and Labor Relations, Cornell University.

Howard D. Marshall: associate professor of economics, Vassar College.

Lester B. McAllister, Jr.: professor of economics, Beloit College.

Simon Naidel: professor of economics and acting chairman, department of economics, The American University.

Alek A. Rozental: associate professor of economics, Saint Louis University.

Nathan Schmukler: associate professor of economics, Brooklyn College.

Irvin Sobel: professor of economics, Washington University.

Jack E. Thornton: Julian Price fellow in insurance, School of Business Administration, University of North Carolina.

Paul Wonnacott: assistant professor of economics, Columbia University.

Appointments

Peter S. Albin, of Princeton University: instructor, New York University.

Curtis Aller, of Michigan State University: associate professor and chairman, department of economics, San Francisco State College, effective September 1959.

Claude A. Bitner, of Michigan State University: assistant professor, department of economics, Texas A & M College, effective September 1959.

Albert A. Blum: assistant professor, New York State School of Industrial and Labor Relations, Cornell University.

Emile Bouvier, of Georgetown University: president of the University of Sudbury, Sudbury, Ontario, January 1959.

Richard S. Bower, of Cornell University: assistant professor of economics and business administration, Vanderbilt University.

John A. Brittain, of Cornell University: assistant professor of economics, Vanderbilt University.

Philip W. Cartwright: associate dean of the College of Arts and Sciences, University of Washington.

J. K. Charles: lecturer in economics, McGill University, 1958-59 session.

Robert W. Clower: chairman of the department of economics, Northwestern University.

Robert P. Collier: acting dean, College of Business and Social Sciences, Utah State University.

Darwin W. Daicoff: instructor, University of Kansas.

Paul M. Dauten, Jr., University of Illinois: a director of Educational and Technical Consultants, Inc., Evanston, Ill.

Richard G. Davis: instructor in economics and research assistant, Princeton University.

Edgar O. Edwards: professor of economics, Rice Institute.

Jose Encarnacion, Jr.: instructor in economics, Princeton University.

Tibor Fabian: director, operations research, Lybrand, Ross Bros. and Montgomery, Philadelphia.

David Fand, of the University of North Carolina: associate professor of economics, Southern Methodist University.

James M. Folsom, of Vanderbilt University: instructor in economics, Duke University.

Paul Fox: research associate in business administration, Harvard Graduate School of Business Administration.

Richard B. Goode, of International Monetary Fund: senior staff member in charge of long-range program of research on economics of taxation, Brookings Institution.

Jack T. Guenther, of Harvard University: assistant professor of economics, Vanderbilt University, September 1959.

William Haber, of University of Michigan: elected president of Industrial Relations Research Association.

John Haldi: assistant professor of economics, Graduate School of Business, Stanford University.

George R. Hall: acting assistant professor of economics, University of Virginia, 1959-60.

Alvin H. Hansen, emeritus, Harvard University: visiting professor of economics, Haverford College, first semester 1959-60.

George W. Hardbeck, of Louisiana Polytechnic Institute; assistant professor of economics, Saint Louis University.

John D. Helmerger: lecturer in economics, University of Minnesota.

Benjamin H. Higgins, of Center for International Studies, Massachusetts Institute of Technology: professor of economics, University of Texas.

Leonid Hurwicz, of the University of Minnesota: visiting professor in economics, Stanford University, spring quarter 1959.

James James: lecturer in economics, Southern Methodist University.

Alexandre Kafka: acting associate professor of economics, University of Virginia.

Thomas F. Keller, of the University of Michigan: assistant professor, department of economics and business administration, Duke University.

Nathan Keyfitz, of Dominion Bureau of Statistics: professor in department of political economy, University of Toronto.

Charles Kretzschmar: economic statistician, Bureau of the Census, Washington, D.C.

Harold Kuhn, of Bryn Mawr: associate professor of mathematical economics, Princeton University.

Paul F. Lazarsfeld: Ford visiting professor, Harvard Graduate School of Business Administration.

Abba P. Lerner: professor, department of economics, Michigan State University.

Eugene M. Lerner: assistant professor, department of economics, The City College of New York.

Nelle P. Lewis: research associate, Bureau of Business Research, University of Kentucky.

Wallace Lovejoy, of Continental Oil Company: assistant professor of economics, Southern Methodist University.

David L. Lutin: member of staff of the Committee for Economic Development, Area Development Division

George Macesich: assistant professor of economics, Florida State University, September 1959.

Allan B. Mandelstamm, of Northwestern University: assistant professor of economics, Vanderbilt University.

Theodore Mesmer: member of staff of the Research and Planning Division, U.N. Economic Commission for Asia and the Far East, Bangkok, Thailand.

C. Clyde Mitchell, of Harvard University Advisory Group to Government of Pakistan: chief of the FAO Mission in Colombia, March 1959.

Leon Moses: associate professor of economics, Northwestern University.

Richard F. Muth: associate professor of urban economics, School of Business, University of Chicago.

Herman L. Myers: program officer and economist, International Cooperation Mission in Mexico City.

I. D. Pal: lecturer in economics, McGill University, 1958-59.

James Pikl, Jr.: assistant professor of economics, Southern Methodist University.

C. Hock Quan, of Carthage College: research associate, Bureau of Business Research, University of Kentucky.

Robert F. Risley: acting dean of the New York State School of Industrial and Labor Relations, Cornell University.

Herbert R. Rouse: visiting consultant on statistics, Harvard Graduate School of Business Administration.

Warren Samuels, of Georgia State College: assistant professor of economics, University of Miami School of Business Administration.

Eric Schenker, of Michigan State University: appointment at University of Wisconsin, Milwaukee branch.

Charles L. Schultze, formerly senior staff Council of Economic Advisers; lecturer in economics at Indiana University.

Mark B. Schupack: instructor in economics, Brown University.

Richard T. Selden, of Vanderbilt University: associate professor of banking, Graduate School of Business, Columbia University, effective September 1959.

C. Joseph Sequin, of Michigan State University: assistant professor, department of business organization and management, Notre Dame University, effective September 1959.

Barbara A. Simpson: instructor in economics, College of William and Mary, effective September 1959.

Frank I. Stern, formerly Grey Advertising Agency, Inc.: now market analyst, New York, New Haven and Hartford Railroad.

George Sternlieb: research associate in business administration, Harvard Graduate School of Business Administration.

Walter F. Stettner, of International Cooperation Administration: senior economist, Council of Economic Advisers.

Albion G. Taylor, professor emeritus College of William and Mary: visiting professor, Hollins College 1959 spring semester.

Ronald L. Teigen, formerly St. Olaf College: instructor, University of Minnesota.

Carl E. Veazie: associated with Ebasco Services, Inc., in department of facilities, community, and industrial planning.

Barton Westerlund, of University of Arkansas: assistant director of research and senior industrial specialist at University's Industrial Research and Extension Center, Little Rock.

William V. Wilmot, Jr.: associate professor of management and head of the department of management and business law, University of Florida.

Tom Wise: lecturer in economics, McGill University, 1958-59.

Leaves for Special Appointments and Assignments

Walter Adams, Michigan State University: visiting professor Salzburg, Austria, Seminar and visiting professor, Centre d'Etudes Industrielles, University of Geneva, spring 1959.

Richard C. Bernhard, University of California, Riverside: guest professor, University of Freiburg im Breisgau, May to July 1959.

Rudolph Blitz, Vanderbilt University: research appointment at Johns Hopkins University, spring semester 1960.

Henry Briefs, Georgetown University: second-year leave to continue in Council of Economic Advisers to the President.

M. P. Catherwood, New York State School of Industrial and Labor Relations: industrial commissioner, New York State Dept. of Labor, effective January 1959.

David C. Cole, Vanderbilt University: assigned to University of Philippines for year beginning February 1960 under Vanderbilt University's Overseas Professorship Program.

Harold W. Davey, Iowa State College: visiting professor in industrial relations, University of Minnesota, spring quarter 1959.

Corwin D. Edwards, University of Chicago: member staff Brookings Institution to undertake study on International Comparative Analysis of Trade Regulation, year 1959-60.

Frederick C. Joerg, Duke University: Fulbright lecturer Helsinki School of Economics, Finland, 1959-60.

John P. Lewis, University of Indiana: member staff Brookings Institution to undertake study on The United States and Indian Economic Development 1959-60.

Richard W. Lindholm, University of Oregon: in Korea as economic consultant to ICA with reference to establishment of an economic development council in Korea, early 1959.

Lawrence C. Lockley, University of Southern California: visiting professor of marketing, Graduate School of Business, Columbia University 1959-60; has resigned deanship of School of Commerce, University of Southern California.

Fritz Machlup, Johns Hopkins University: lecturer at the Universities of Cologne, Bonn, Mainz, Frankfurt, Munich, Freiburg and Saarbrücken, January-February 1959.

Harry McAllister, State College of Washington: research associate with National Bureau of Economic Research.

James W. McKie, Vanderbilt University: visiting associate professor of economics, Harvard University, 1959-60.

David N. Milstein, Rutgers—The State University: on research project in the Fiscal and Financial Branch, United Nations Secretariat, past year.

Carl L. Nelson, University of Minnesota: visiting professor, School of Economics in Turku, Finland, and Fulbright fellow, 1959-60.

Henry H. Schloss, Washington University: Fulbright lectureship at the University of Bombay.

John Sheahan, Williams College: Brookings Institution National Research Professorship for research in France, 1959-60.

Jack Stieber, Michigan State University: guest professor in department of industrial administration, Royal College of Science and Technology, Glasgow University, Scotland.

Charles J. Stokes, Atlantic Union College: Fulbright professor of economics at the University of Guayaquil and the Central University of Ecuador, 1958-59.

Philip Taft, Brown University: visiting professor, University of California at Berkeley, second semester 1958-59.

Anthony M. Tang, Vanderbilt University: assigned to the Institute of Social and Economic Research, Osaka University, Japan 1959-60 under Vanderbilt University's Overseas Professorship Program.

Gerald E. Thompson, University of Nebraska: appointed by Ford Foundation fellow of Institute of Basic Mathematics for Application to Business to be held at Harvard and Massachusetts Institute of Technology, 1959-60.

Gerhard Tintner, Iowa State College: taught econometries during spring quarter at Technical University, Lisbon, Portugal, at request of U.S. Department of State.

Walter H. Zukowski, Colby College: visiting professor of business administration, Al-Hirma University of Baghdad, Iraq, under Rockefeller Foundation grant for year 1958-59.

Resignations

Frank T. Bachmura: Vanderbilt University.

John M. Baitsell: Harvard Graduate School of Business Administration.

Manuel Gottlieb: University of Kansas.

William A. Kamins: Harvard Graduate School of Business Administration.

Richard W. Kazmaier, Jr.: Harvard Graduate School of Business Administration.

Robert A. Solo: The City College of New York.

Maxine Woolston: Bryn Mawr College.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the Review will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Economics and business administration: A Christian liberal arts college will have a vacancy September, 1959, for someone to handle courses in economics, business organization and finances, salesmanship, to be followed second semester by economics, marketing, and business management. Desire Ph.D. but master's degree with teaching experience will be considered. Salary and rank are open, depending upon training and experience. Write Chairman, Department of Economics and Business Administration, Geneva College, Beaver Falls, Pennsylvania.

Marketing, finance, and management: Unusual opportunities for competent teachers able to work with business groups. Two openings, from assistant to full professor. School of business administration in Washington, D.C. P214

Business law and accounting: School of business administration in the nation's capital has opening for instructor or assistant professor, LL.B. and accounting B.S. required minimum. For 1959-60 academic year. P218

Business law: Opportunity for assistant or associate professor of business law. Qualifications in business-government relationships highly desirable. School of Business Administration in nation's capital. P219

City planning agency: Associate research planner position open to economist with master's or bachelor's degree and a year of experience in capital programming and planning analysis (economic and population data). Starting salary $6,590 with annual increments of $200 plus cost-of-living bonus. Write to Arthur J. Reed, Commissioner of Planning, Department of City Planning, 211 East Water Street, Syracuse 2, New York.

Accounting: Senior-level position at midwest university. Desired qualifications are: doctoral degree, C.P.A. certificate, teaching experience at the university level, and publications. Salary and rank commensurate with qualifications. P220

Economists Available for Positions

International economics and international relations, economic policy and government in economics, corporation finance (American and international), public finance, economic history, economic geography, comparative systems, regional economics of Europe, Middle East, Latin America: Man, in fifties; U.S. citizen; European professorial qualification; doctoral degrees in economics, finance, and law. Head of a department, full and visiting professor on four continents, including a leading university; 5 years of teaching at a U.S. university; research director; international bank-